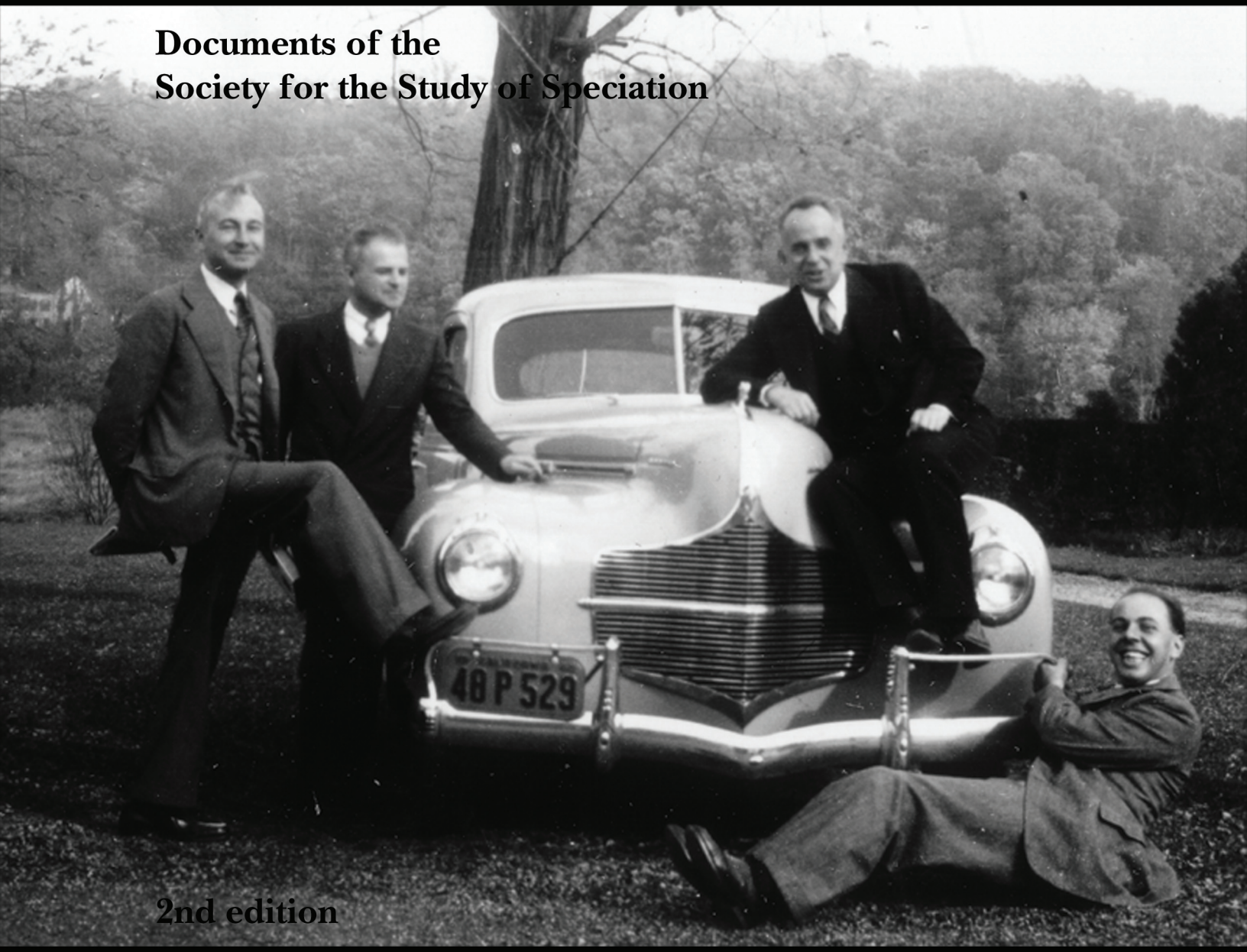
regular contact with anyone interested

Documents of the Society for the Study of Speciation



2nd edition

Joe Cain, editor

regular contact with anyone interested

Documents of the
Society for the Study of Speciation
2nd edition

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editor

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cover photo: "The Dobzhansky's beautiful new car, completely surrounded by big shots". Left to right: Kaufmann, Demerec, Dobzhansky, Fano. In Milislav Demerec Papers. Photobook titled, "Remembrance of things past in the summer of 1940" (U5-2.40.37). Reproduced with permission. American Philosophical Society Library.

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Preface

The Society for the Study of Speciation has been referred to many times, but little was known about it when I began research in the early 1990s into organisations related to the synthesis period in evolutionary studies. The documents reproduced here certainly were forgotten. As they carried no masthead, were intentionally informal, and were the only ones of their kind, it was easy for them to become lost. During my research on professional societies, the real meaning of Emerson's documents was rediscovered.

The *Society for the Study of Speciation* formed in December 1939 at the Columbus meeting of the American Association for the Advancement of Science. It started with a discussion between Julian Huxley and Theodosius Dobzhansky, and probably also Ernst Mayr. Afterwards, they included Alfred Emerson, asking him to serve as secretary for the Society.

Emerson's first step was to publicise the organisation. First he used notices in journals. These announcements began to appear in March 1940. A circular also was posted to prominent colleagues.

The documents in this collection – a *News Bulletin*, Address List, and Bibliography – were the only publications produced under the Society's name. Emerson published these in his role as secretary. They were posted as a package in March 1941, sent both to members and to others Emerson thought might be interested.

Originals of the *News Bulletin* and Address List came into my possession as a gift from Professor Robert E. Sloan (University of Minnesota). Sloan received them from Professor Everett Claire Olson, his mentor at the University of Chicago. I have since deposited copies of these documents, in Sloan's name, in the American Philosophical Society Library (Philadelphia, PA); see 'Society for the Study of Evolution Records', series 8. In later work elsewhere, I've discovered additional copies and have done my best to bring these to the attention of archivists for re-cataloguing. In 2001, Kim Kleinman reported another copy of the *News Bulletin* in the Ewan Collection, Missouri Botanical Garden Library. The *Bibliography* used for this project is located in the 'Society for the Study of Evolution Records' at the American Philosophical Society Library, previously in a miscellaneous collection of 'early letters' held by Ernst Mayr. Sadly, no material relating to the speciation society is preserved in the sparse Alfred E. Emerson Papers (University of Chicago Special Collections Research Center).

Thanks to the following people for their assistance in this project: Robert E. Sloan, Rita Dockery, John Beatty, Michael Ruse, and staff at the American Philosophical Society Library. Thanks also to the Society for the Study of Evolution for permission to reprint these documents.

The 1999 Riverside Press edition of this book was privately circulated and long out-of-print. This second edition includes numerous revisions and clarifications.

Joe Cain
London 2007

Introduction

Organising the *Society for the Study of Speciation* was a simple affair. It occurred at the 1939 annual meeting of the American Association for the Advancement of Science (AAAS) in Columbus, Ohio, and it was largely *impromptu*.[1] Ernst Mayr later described the moment as a 'conference between [Julian] Huxley and [Theodosius] Dobzhansky' at the Columbus meetings that he too 'sat in at' in which the basic scheme was drawn.[2] Several days later, still in Columbus, some combination of this trio approached Alfred Emerson with the goal of implementing some kind of plan. They hoped to persuade Emerson to take charge of the group. Neither Huxley nor Dobzhansky wanted the administrative burden.[3]

These principals agreed among themselves there now existed in speciation studies 'an informal co-operative group of scientists willing to pass information from one to the other.' Something was needed, they agreed, to simplify exchange of information and to enrol others in the sharing process. Too much was happening, and even they had difficulty keeping up.

Emerson agreed to take on the job. 'The need was felt by many students of speciation for a greater degree of integration between the various fields,' he wrote when announcing the group's creation.

> Those contributing to an understanding of the factors influencing speciation are often in fields and institutions which have little direct contact with those who are attacking the problem from somewhat different angles and are using different techniques….The general object of the Society [will be] to institute an informal information service which will tend to correlate the various approaches.[4]

Implementing a plan – actually forming a group and organising its activities – fell into Emerson's hands as society 'secretary'. At the Columbus meeting, he either agreed or volunteered[5] to serve as secretary

[1] The launch was not reported in the meeting summary published in *Science*, see: G. Baitsell, E.G. Butler, E. M. Cory, C. Mickel, and O.R. McCoy, [Reports of 1939 meetings for Section (F) and associated societies], *Science* (1940), 91: 110-119.

[2] Mayr to Emerson, 26 March 1940, f: 75, Papers of Ernst Mayr, collection HUG(FP) 14.7, Professional Correspondence, 1931-1952, Harvard University Archives, Cambridge, MA (hereafter *Mayr-Harv*). On Emerson's role, also see Emerson to Colleagues, 18 March 1940, in f: 'Emerson, R. A.', Papers of Leslie Dunn, collection B-D917, American Philosophical Society (hereafter *Dunn*). Mayr later said Carl Epling also attended the conference with Huxley, see 'History of the Society for the Study of Evolution,' Society for the Study of Evolution Records, series 8, American Philosophical Society. Correspondence prior to the meeting between Huxley, Dobzhansky, and any other principals on organising this groups has seen been lost.

[3] For reasons that are unclear, they chose not to ask Mayr if he would be interested in the job. Probably, seeing him in 1939, Mayr lacked gravitas. On Mayr and this issue, see Cain, Joe. 2002. Epistemic and community transition in American evolutionary studies: the 'Committee on Common Problems of Genetics, Paleontology, and Systematics' (1942-1949). *Studies in History and Philosophy of Biological and Biomedical Sciences* 33: 283-313.

[4] A. Emerson, [Excerpts in 'Evolution News'], *American Naturalist* (1941), 75, 86-89.

[5] Emerson said he 'was approached' by Huxley and Dobzhansky 'with the suggestion that he organise a co-operative

indefinitely, or at least until 'the permanence of this society is assured' and 'a system of election by members' could be introduced.[6] As sole officer (other than an *ad hoc* 'executive committee' that had no stated function), Emerson was responsible for the 'general organisation of the group' and the 'publication of information.'

Emerson's first step as secretary was to distribute a questionnaire announcing the group's formation: seeking interested parties and asking for a first round of news and commentary. He posted this in March 1940, telling colleagues that this 'loose organisation of co-operating members' would sponsor 'the publication of a booklet about twice a year for spreading pertinent information concerning bibliographical citations and notes from various members and laboratories.'[7] Emerson wanted specialists to send him correspondence about their research: 'notes concerning original work, critical comments upon the work of others, and news items of interest to the group.' To this correspondence he would add a few pieces of his own or add items he had solicited from others. All this would be distributed to 'anyone interested'.

Despite strong initial interest, more than a year elapsed after Emerson's original notice before he posted the first batch of material in March 1941.[8] As expected, the 29-page *News Bulletin* was an informal product – mimeographed with neither cover page nor masthead. It included 'notes and comments' from a few dozen researchers – a hodgepodge of queries, statements, and suggestions for research projects excerpted from incoming letters. It also included roughly thirty 'communications from laboratories, organisations, and individuals' describing ongoing research, plus a set of comments collectively described as a 'discussion from members concerning [a] statement of the objectives of the Society for the Study of Speciation.' This last section included both encouragement and suggestions about the Society's domain.

In its original form, the bulletin bore strong resemblance to other informal newsletters the principals were well acquainted with, including the *Maize Breeders Co-operation News Letter* (popularly called the 'corn letter,' begun in 1932), the *Drosophila Information Service Bulletin* (1934), and the *Society of Vertebrate Paleontology News Bulletin* (1940).

To the first bulletin, Emerson added notes about the scope and organisation of the Society – these were reprinted in *American Naturalist*.[9] As an example of the kind of dialogue he hoped to facilitate, Emerson also added a deliberately provocative, 5,900-word review of *The New Systematics*, then just available in the United States. Of this anthology he wrote, '…[T]his work is an important contribution to the difficult task of welding many techniques of investigation pertaining to the central problem of evolutionary dynamics.'[10]

Accompanying the bulletin was Emerson's first instalment of a bibliography on speciation and an address list

association of individuals…' in Emerson to Colleagues, 18 March 1940, *Dunn*, f: 'Emerson, R. A.'.

[6] Emerson, *op. cit.* (4), also see Emerson to Colleague, 18 March 1940, *Dunn*, f: 'Emerson, R. A.'.

[7] Emerson to Colleague, 18 March 1940, *Dunn*, f: 'Emerson, R. A.'. (This announcement is misfiled under *Ralph* Emerson and not *Alfred* Emerson.) This announcement comes close matching to what Huxley claimed to his friend John Gilmour, saying that he (Huxley) had arranged for the 'issuing of a mimeographed bulletin, twice yearly, giving lists of the more important papers on zoological and botanical systematics which would interest workers in cytogenetics, ecology, and other branches of biology, and visa versa, together with notes and queries, notes on methods, etc. Prof. A. E. Emerson is undertaking the work, with the aid of strong local and general committees.' Attached note on Huxley to Gilmour, 12 March 1940, Julian Huxley Papers, Rice University, box 14, f: 2.

[8] Undated, external evidence places the news bulletin's release to March 1941. The only reaction to the bulletin located to date is in March 1941, see: Mayr to Emerson, 31 March 1941, *Mayr-Harv*, f: 75; and correspondence with Huxley and colleagues in June 1941, *Huxley*, box 15, f: 3.

[9] Emerson, *op. cit.* (4).

[10] Emerson, [*News Bulletin*], 18.

of members. Emerson wanted to publish bibliographies related to all aspects of the Society's domain. He searched the literature himself. He also asked members to contribute citations of their own. For intellectual control, Emerson developed a indexing system (see page 103 in this volume). Along the same practical lines, he had the bibliography printed 'in such a manner that the items may be cut out and pasted on catalogue cards if desired.' Listing 1,250 entries, this bibliography offers historians a superb sample of the speciation literature during the late 1930s. Emerson's key to classification also offers a fascinating intellectual division for speciation studies at the time. However, despite the massive number of references, the bibliography's coverage was uneven. Some contributors sent in their entire publications list regardless of their relevance to speciation. Others were overly selective.

Emerson recognised the bibliography's shortcomings, so he asked members for assistance. 'The society is formed to pass information among members, but the members should take the responsibility for placing such information in the hands of the secretary…' (p. 29) First on his desired list was 'titles of important publications with appropriate bibliographical citations….These should include important items missed in the former lists.'[11]

Emerson's address list included 374 individuals. A study of a forty percent random sample of this membership suggests their basic distribution. In age, the membership was skewed significantly toward the early-middle portions of careers,[12] though otherwise the distribution was roughly normal. A majority of members in the sample were members of AAAS (sixty-eight percent). Membership in other umbrella organisations most frequently included the American Society of Zoologists (twenty-seven percent), the Genetics Society of America (twenty-four percent), the American Society of Naturalists (twenty-two percent), the Botanical Society of America (eighteen percent), and the Ecological Society of America (eighteen percent); overlap was high in these memberships.[13]

Member specialities clustered in roughly the same way membership in umbrella organisations did. The vast majority claimed expertise as taxonomists, though organismic biologists (ornithologists, mammalogists, botanists, etc.) outnumbered geneticists and cytologists roughly two to one. Of additional note is the unexpectedly high frequency of entomologists and the substantial additional range of research specialities represented in the sample – from palaeontologists to horticulturists, bacteriologists to physical anthropologists. These demographics basically match the skewed distribution for the 'executive committee,' where two geneticists and two cytologists were outnumbered by seven zoologists and one palaeontologist. The membership sample also was well distributed across the United States, with a small number from Canada and Britain.

In March 1941 the Society seemed off to a solid start. All indications suggested strong momentum, high participation, and an eager audience. Speciation workers could now look forward to increased communication and years of productive interaction.

Such optimism proved unwarranted, however. The Society quickly fell apart. The first bulletin was the last. The bibliography stopped. The address list never underwent revision. By 1942, the Society, at best, could be described as 'quiescent.' Others simply called it dead.

[11] Emerson, [*News Bulletin*], 29.

[12] The age distribution followed the following sequence: younger than thirty, 8 percent; between thirty and forty, 35 percent; between forty and fifty, 23 percent; between fifty and sixty, 21 percent; sixty and over 13 percent.

[13] Membership in other organisations was determined using those identified by persons listed in *American Men of Science*, 7th ed. (1944). An important point to note is that memberships in these organisations overlapped significantly and sometimes clustered strongly. For example, given membership in the speciation group and the Botanical Society of America, chances are high that the person also was a member of the Genetics Society of America.

The cause of this collapse is multi-faceted. When the Society was founded, Emerson agreed to serve as secretary – to be 'responsible for the general organisation of the group' and the 'publication of information for distribution.'[14] But Emerson was a busy man in these years, and other priorities forced the speciation society low on his list. In addition to extensive teaching and research, he was expanding his concepts of social co-ordination and the superorganism – this in the context of the global rise of fascism and considerable debate over the proper role of individuals in a society. Emerson also was ending his second term as editor of *Ecology*, writing innumerable commentaries and reviews, as well as serving several other professional societies in administrative capacities.

On top of this, Emerson carried a heavy administrative load in the University of Chicago's active ecology and zoology groups. He also had recently begun a major collaborative book project, with weekly meetings to meticulously read each developing chapter.[15] '…I have too much to do…' Emerson regularly complained.[16]

Emerson's complaints about workload crept into in his first bulletin. Though he asked for, and tried to respond to, ideas from members, 'a number of suggestions, although laudable, involve more time than the secretary can devote to this undertaking…'[17] Always working '…within the limits of his time…' Emerson also apologised when the Society's abstracting project proved too involved. 'Time from other activities did not permit a more adequate classification or abstracting of the literature which amounted to more than was anticipated.' And he apologised for the 'numerous typographical errors' that could not be 'adequately proof-read in the time available.'[18] After the first bulletin had been posted and comments came back to Emerson, he found he could not keep up with demand. 'The difficulties [facing the Society] are purely a matter of the time involved…,' he explained.[19] Emerson was overwhelmed, and it showed. It did not help that he put the full weight of the society on his own shoulders.

Precisely how and how much national mobilisation and America's late entry into the 1939-1945 war affected the Society's plans more generally is not clear. However, many specialists put research programmes on hold (or adapted them or decreased their attention to them) in favour of those relating somehow to the wartime situation. Others entered military service or took on supporting roles, leaving their scientific careers temporarily on hold. With this disruption and dislocation going on everywhere around him, it is likely Emerson followed the same strategy: putting the Society and its publications on hold and setting himself on to more pressing work.

Besides timing, another problem inherent in this fledgling group was its extremely wide range of interests. Watching from England, William Turrill noticed this, suggesting to Huxley that 'our American friends are finding the title of their Society too narrow.'[20] The wide range – from bacteriology to physical anthropology, from *Drosophila* salivary gland chromosomes to Pleistocene glaciation – made it difficult for any secretary to

[14] Emerson, [*News Bulletin*], 1.

[15] E. Wilson, and C. Michener, 'Alfred Edwards Emerson', *Biographical Memoirs of the National Academy of Science* (1983), 53, 159-175; G. Mitman, 'From Population to Society: The Cooperative Metaphors of W. C. Allee and A. E. Emerson', *Journal of the History of Biology* (1988), 21, 173-194; G. Mitman, *The State of Nature: Ecology, Community, and American Social Thought, 1900-1950*, Chicago, 1992.

[16] Emerson to Simpson, 6 November 1946, George Simpson Papers, collection 31, series 1, f: 'Emerson, A. E'. American Philosophical Society Library.

[17] Emerson, [*News Bulletin*], 2.

[18] Emerson, [*News Bulletin*], 29.

[19] Emerson to Mayr, 30 April 1941, *Mayr-Harv*, f: 75.

[20] Turrill to Huxley, 12 June 1941, *Huxley*, box 15, f: 3.

maintain a clear focus for the Society or to preserve the sense of unity and common purpose. Too many constituencies pulled the society in too many directions.

As described in the first bulletin, the group's founders hoped to create a communication network for those interested in the 'dynamics of the origin of species.' Emerson and other society principals understood this to mean the mechanics of speciation processes, such as were discussed in the Dobzhansky-organised symposium at the 1939 AAAS meeting in Columbus. But others had different interpretations of what the Society offered. The physical anthropologists wanted to discuss how best to distinguish human races. The bacteriologists wanted operational species concepts for the test-tube environment. Some members wanted to discuss specific phylogenies; others wanted to examine the origins of particular adaptations. Breadth stretched the identity of the infant Society to its breaking point.

A large number of taxonomists with expertise in the morphology and geographical distribution of particular plant or animal groups joined the Society, though they seem to have had little interest in the dynamics of speciation processes per se. Their interest in joining – based on sample investigations of their careers and on comments published in Emerson's first bulletin – involved the new systematics (*sensu* Huxley) more than basic investigations of speciation mechanics. For the most part, systematists in the Society simply wanted techniques to improve their classifying. Rather than investigating processes and mechanisms of species formation or hoping this new group would create some sort of sub-speciality within speciation or evolution, these taxonomists asked questions such as: how can we define a species *vs.* subspecies *vs.* varieties with less subjectivity? how much and what kinds of divergence offer diagnostic tools when identifying subspecies? how do we integrate cytological and genetic results with the morphological criteria we already use for taxa? Though complex issues in themselves, these taxonomists represented such questions as something other than 'theoretical' issues regarding species formation.

This cluster of interests among systematists was different from those of the Society's principals in important ways. The principals wanted to extend taxonomic work and extract from it information about biological processes and mechanisms. In many ways, this cluster would have been wholly satisfied with an organisation more like what Huxley had been involved with in Britain, the 'Association for the Study of Systematics in Relation to General Biology,' later the Systematics Association.[21] They would not have been well served with the Society as planned at the AAAS meeting and implemented by Emerson.

Criticisms of the first bulletin added to overall dissatisfaction. Anxious to facilitate research in this area as best he could, Huxley circulated copies of the bulletin and bibliography to British colleagues soon after these publications arrived in London. Uniformly, those colleagues complained about the inadequacy of the bibliography compared with the *Zoological Record*, published regularly by the Zoological Society of London. *Zoological Record* was the international standard for taxonomic literature. Both Owain Richards and John Smart complained to Huxley that they thought the Americans were making poor use of it. Smart announced his intention to work on this problem after the war. Although he too was sceptical of the bibliography, William Turrill managed to find a constructive note, '…I should not wish to be in any way unappreciative of Emerson's efforts – we have found some of his references useful already…'[22]

In sum, trying times, a swamped editor, the heterogeneity of the group, and this general dissatisfaction with the Society's initial projects brought about the group's ungluing.

Activities in New York quickly made the Emerson's activities redundant. By 1941, Ernst Mayr was focusing

[21] Winsor, M.P. 1995. The English Debate on Taxonomy and Phylogeny, 1937-1940. *History and Philosophy of the Life Sciences* 17:227-252.

[22] Turrill to Huxley, 12 June 1941, *Huxley*, box 15, f: 3 and related correspondence in same folder.

the majority of his intellectual attention onto speciation studies. With boundless energy, he had no patience for quiescence or complaints and overloading. Mayr frequently complained about Emerson's failure to seize initiative. He began working to sideline Emerson in favour of more 'active' workers. First, Mayr attempted to replace Emerson as secretary, but Dobzhansky restrained him. Next, Mayr took an alternative track. By 1943 he had gained administrative control over the Committee on Common Problems of Genetics and Paleontology, a committee organised within the National Research Council by palaeontologists interested in collaborating with geneticists. Mayr transformed this committee into an effective piece of infrastructure for the wider community of interest in speciation studies. This eventually expanded into a professional society (the Society for the Study of Evolution, founded 1946 with Mayr as Secretary) and journal, *Evolution* (volume 1 is 1947; Mayr was the journal's first editor). Aside from the occasional polite historical reference to Emerson and the speciation society, Mayr never looked back. He had made Emerson's project obsolete.

Further reading

A more complete account of the speciation society is available in:

Cain, Joe. 2000. Towards a 'greater degree of integration': The Society for the Study of Speciation, 1939-1941. *British Journal for the History of Science* 33: 85-108.

An analysis of the Mayr's role in the end of the Society for the Study of Speciation is available in:

Cain, Joe. 2002. Epistemic and community transition in American evolutionary studies: the 'Committee on Common Problems of Genetics, Paleontology, and Systematics' (1942-1949). *Studies in History and Philosophy of Biological and Biomedical Sciences* 33: 283-313.

News Bulletin

Editorial notes for *News Bulletin*

Spelling in the *News Bulletin* follows the original. Italics here replace underlining in original. Information in [ ] brackets adds editorial insertions by Cain. Information in ( ) parentheses was added by the *News Bulletin*'s editor, Emerson. Bracketed numbers mark page transitions in original – *e.g.*, [3|4] marks the transition between pages 3 and 4. By convention, generic and species names should be italicized; here, this occurs only when appearing in the original. The presence of paragraph starts at page transitions is not obvious in the original. Cain has interpreted these here. Names have been made as complete as possible following *American Men of Science* (see editorial notes for Address List).

In reproducing some of the correspondence in this *News Bulletin*, Emerson was not clear if he was providing verbatim transcriptions of correspondence or was giving readers summaries of correspondence.

Objects of the Society for the Study of Speciation

The need is felt by many students of speciation for a greater degree of integration between the various fields. Those contributing to an understanding of the factors influencing speciation are often in fields and institutions which may have little direct contact with those attacking the problem from somewhat different angles and using different techniques. Bibliographies and workers are scattered.

The general object of the society is to institute an informal information service which will tend to correlate the various approaches.

Fields of Interest

The major field of interest is the dynamics of the origin of species. Obviously the analysis of the factors of speciation involve the study of divergence of populations classified as subgroups within the species. Therefore studies of the origin of local populations, races, and sub-species are necessary parts of the study of speciation. Also many factors may be studied and verified through analysis of the evolution and stability of the higher taxonomic categories. There should be no limitation on the inclusion of any phase of evolution that contributes to an understanding of the central problem of the origin of species.

The major factor complexes may be termed hereditary variation, isolation and selection. These may be

subdivided into various types and mechanisms and numerous illustrative examples among plants and animals may be given. The recognized fields of Bacteriology, Botany, Zoology and Anthropology have long been interested in the species problem. The biological sciences which obviously are making contributions to speciation and general evolution include Morphology, Cytology, Genetics, Biogeography, Ecology, Paleontology, Physical Anthropology, Comparative Psychology, Comparative Physiology, Embryology, Population Biology and Taxonomy.

Organization

A group of scientists from various fields have joined an informal society called "The Society for the Study of Speciation." This society has a secretary and an executive committee. The secretary is responsible for the general organization of the group, and the publication of information for distribution. The various members will contribute this information. The publication program includes bibliographies, notes concerning original work, critical comments upon the work of others, and news items of interest to the group. Anyone interested in receiving the publications should join the society by sending in his name and filling out the questionnaire. Anyone wishing to contribute information for the booklets should send material to the secretary.

At the present time the following executive committee is functioning for the society:

- Edgar Anderson
- John M. Beal
- William Burrows [1|2]
- L[eon]. J[acob]. Cole
- L[ee]. R[aymond]. Dice
- Th. Dobzhansky
- Alfred Emerson (Secretary)
- A[lfred]. C[harles]. Kinsey
- W[ilton]. M[arion]. Krogman
- Karl P[atterson]. Schmidt
- George G[aylord]. Simpson
- Sewall Wright

When the permanence of this society is assured, a system of election by members will be instituted.

It is not desired that one more formal organization be added to the large number already in existence, but the need is felt for an informal cooperative group of scientists willing to pass information from one to the other.

Through an anonymous donation, the initial expenses of organization have been paid for. It is contemplated that the society will become self-sufficient soon after it is organized, and the first information booklets have been received. However, the expenses should be kept at a minimum to cover the small costs involved in the issuing and the mailing of the booklets. It is suggested that the simplest and least expensive form of publication will serve the needs of the members best.

The original suggestion for organizing such a society in the United States came from Julian Huxley who conferred with numerous individuals in this country. The need for such a society had been realized for some time and various local groups had already organized, as well as sections and committees of existing societies. It is hoped that this society can cooperate with these groups, and extend its range to include scientists and organizations in various countries as conditions permit.

The secretary will attempt to help in coordinating the various fields and individuals. Many suggestions have already been received which deserve careful consideration, and a cross section of opinion is included in this

booklet. A number of suggestions, although laudable, involve more time than the secretary can devote to this undertaking, or involve more of a financial structure than seems possible at the present time. Other suggestions are feasible but require more general expressions of opinion in order to be put in practice. The secretary, within the limits of his time, will attempt to bring the constructive suggestions before the society for consideration and democratic action.

Notes and Comments

Banta, A.M. [Arthur Mangun - AMS 7: 85]

G. L. Church has some significant material on polyploid races of grasses.

Bates, M. [Marston - AMS 7: 106]

I wish there were some way of establishing a more or less long range study of the "species problem" in some group somewhere in the tropics. It has always seemed to me that difficulties were greatly increased by the seasonal interruptions and comparatively slow tempo of the temperate zone, and work in the tropics has always been scattered and limited in time. [2|3]

Brower, A.E. [Auburn Edmond - AMS 7: 217]

This is a problem for research: A biochemical study of the basic factors involved in the great increase in the number of specimens of melanic *Acronicta*, *Catocala*, and other Lepidoptera taken near certain manufacturing centers, and if this type of melanism affects the offspring?

Doering, K. [Kathleen Clare - AMS 7: 459]

I suggest for consideration:

- Regulation as to the requirements of becoming a taxonomist so there won't be so many small, isolated papers by workers not well equipped with the family and generic groups.
- Standardization of family names by vote of *all outstanding taxonomic authorities* in an order. This would prohibit the splitting of well-known family names into several families which *are* accepted by some workers and *not* accepted by others.
- Urge the speeding up of Zoological Record if possible.

Fosberg, F.R. [Francis Raymond - AMS 7: 588]

The term "speciation" is not only redundant, but, so far as I have talked to its proponents, does not connote a new idea. It is only used by a few of the people who would have information to contribute. Most of the desirable data appears in papers in which the term "speciation" does not occur at all, in discussions of genera, etc. by people who merely bring it in as evidence in support of their other problems, and who would certainly be horrified at the idea of writing a paper on "speciation."

Gates, R.R. [Reginald Ruggles - AMS 7: 630]

I used the term *speciation* in a paper in *Amer. Nat.* about 1917 and have always wondered whether someone else had used it before me.

Granovsky, A.A. [Alexander Anastacievitch - AMS 7: 680]

It seems that the phylogeny of insects can be better determined by some modified precipitant tests.

Hadley, P. [Philip Bardwell - AMS 7: 715]

> [Emerson's excerpting] …bacteriologists, at the present time, do not even know what a bacterial species is. They do not even know what constitutes the bacterial *individuum*. And the unfortunate part of the situation is that, preoccupied with problems of application, the majority of bacteriologists are not concerned with these matters that are of fundamental importance to the science of bacteriology – if such a thing may be said to exist.

Howell, B.F. [Benjamin Franklin - AMS 7: 855]

> Cooperation between paleontologists and other biologists in the study of Pleistocene climate's influence on evolution and the development of new species would probably yield interesting results. [3|4]

Lindsey, A.W. [Arthur Ward - AMS 7: 1070]

> I have noticed in recent years that no program mentions evolution, although speciation frequently occurs. Since speciation means literally the formation of species by processes of evolution, the implication has been that speakers taking part in these programs were interested only in a limited part of evolution – an obvious absurdity. Now that you specifically mention the inclusion of "the dynamics of races and subspecies" we are back on the old footing; we are interested in organic evolution, not merely speciation. Is it not rather unworthy of a serious scientific group to perpetuate such a specious distinction, and particularly by a misuse of a term?

Communications from Laboratories, Organizations and Individuals

Beasley, J.O. [J. Otis - AMS 7: 115]

> A study is underway on the structural differences between chromosomes of the species of *Gossypium*. Observations are being made on the segregation of colchicine induced allopolyploids.

Boyden, A. [Alan Arthur - AMS 7: 189]

> We have begun a serological study of Drosophila species using a newly developed photoelectric precipitin technique.

Breland, O.P. [Osmond Philip - AMS 7: 203]

> Includes study of the parasites of the egg cases of preying mantids, with particular reference to their distribution, biology, and specificity in parasitic reactions. Two groups of parasites offer particularly interesting opportunities for study. These insects have here-to-fore been considered as the same species, parasitizing two different species of mantids. Preliminary studies show that there are slight morphological differences between the two groups of parasites; that each group probably parasitizes only one species of mantid; that the ranges of the two species are different except in one region where the ranges overlap slightly. Preliminary studies also indicate that despite only slight morphological differences, the insects will probably not interbreed with each other. The problem as to whether or not these insects are the same or different species will be investigated fully in the near future. The region in which the range of the two groups come together will be selected for the study.

Buchanan, R.E. [Robert Earle - AMS 7: 236]

I am a member of the International Commission on Nomenclature of Bacteria appointed by the International Microbiological Congress. We are much concerned with adequate working definitions and terminology for subdivisions of species within this field. We are preparing an International Code for adoption at the next meeting.

Bugbee, R.E. [Robert Earl - AMS 7: 240]

A study *in progress* of Mexican Eurytomidae with description of the new species and classification of them based on morphologic, physiologic, ecologic, and distributional data. [4|5]

Bujak, B.J.

I am working, at least partly, on the behavioral relationship of the two species of Peromyscus under natural conditions here in southern Michigan.

Clausen, J. [Jens Christian - AMS 7: 325]

The Carnegie group (Clausen, Keck and Heisey) has in print the first volume of a planned series of publications on the dynamics of speciation. This is "Experimental studies on the nature of species. I. Effect of varied environments on western North American plants" (Carnegie Institution Publication No. 520). Intensive work is in progress towards the fulfillment of a second phase – the extensive investigations of the Madiinae.

The present volume deals primarily with the adjustments of plants to their environments. In forthcoming publications the internal adjustments of plants, the barriers that separate species, and the various evolutionary patterns are to receive first attention, together with a discussion on the literature dealing with the subject.

Another project in progress is a selection experiment. This investigates the reactions to three climatic complexes of an F2-population of a cross between an alpine and a foothill ecotype of one species, *Potentilla glandulosa*.

It may interest you that since the fall of 1935 we have had in the San Francisco Bay region a very active association like the one you propose. It consists of botanists and zoologists interested in distribution and evolution.

The organization is very simple and democratic, and consists of approximately twenty-five members with neither chairman nor secretary and with no treasury. The group meets once each month for discussion. The speaker of the evening calls the members by postcard, announces the subject and presides over the meeting, which is very informal. A real round table discussion develops during the presentation of the subject by the speaker of the evening.

The members of the group have strong experimental leanings and problems are discussed from taxonomic, morphological, geographical, ecological, cytogenetical and physiological angles. They call themselves Biosystematists, a name rather recently decided upon. The group is composed of members from the University of California at Berkeley, Mills College, Oakland, California Academy of Science, Stanford University and the Carnegie Institution Laboratory at Stanford.

Darlington, Jr., P.J. [Philip Jackson - AMS 7: 409]

I am now especially interested in: the facts, causes, and history of distribution of land animals; the atrophy of the wings especially of Carabidae inhabiting mountains and islands; and the natural dimorphism involving taxonomic characters, especially in Carabid beetles. [5|6]

Dunn, E.R. [Emmett Reid - AMS 7: 485]

It would be well if all members should spread abroad to workers in variation (geographic or of any kind) that they should distinguish in some way between adults and young. A vast amount of work has been done which is vitiated and almost worthless because of this omission. There is clear evidence, in certain groups I have worked with recently, and in some papers in which this datum is given, that adult characters have a significantly different range of variation from those of the young, and this in *characters which are not subject to modification during life*. I have some work of this kind in progress. It produces clear evidence of natural selection, the reproductive population being statistically different from the (shall we say) genetic population.

I have work in progress on the snake populations of Panama which may prove of interest, in that there is a fairly clear correlation between (a) rarity of individuals, narrowness of range, and increase in number of species and (b) abundance of individuals, wideness of range, and increase in number of species. This is contrary to accepted ideas, but as it is based on actual counts of random collections it may be significant.

Dymond, J.R. [John Richardson - AMS 7: 492]

Our Museum is mapping the distribution of animals in Ontario and so far as possible studying the characters (size, color, relative size of body parts) in correlation with distribution. In fishes, problems of relative growth take a prominent place.

Eigsti, O.J. [Orie Jacob - AMS 7: 507]

I have at present an interesting case of amphidiploidy in *Polygonatum* which is under consideration from a cyto-taxonomic point of view.

Enders, R.K. [Robert Kendall - AMS 7: 522]

I am working on speciation of mammals in the Chitiqui region of Panama and comparative embryology of the marsupials.

Errington, P.L. [Paul Lester - AMS 7: 528]

First draft MS on 'Bergmann Rule' and survival of bobwhites under highly adverse conditions at the northern and northwestern fringes of the species range has been prepared but is far from ready for publication; MSS on predation upon, and population mechanics of, Muskrats will be prepared in the future, and these will have a certain bearing on questions of natural selection.

Fox, H. [Henry - AMS 7: 593]

Since 1936 I have accumulated much data on the relative abundance of local populations of species of Orthoptera, including some cases of seasonal and annual fluctuations; also on their habitat distribution in relation to types of vegetation and soil.

In 1938-39 I gathered additional data on egg content and nymphal emergence in oothecae of two introduced species of Asiatic Mantids, as well as in the relative abundance of their oothecae in local areas.

In 1938-39 I conducted experiments testing the effects of modified feeding procedure upon the survival of Japanese beetle larvae at constant high temperature. [6|7]

Gates, R.R. [Reginald Ruggles - AMS 7: 630]

The Monographic work on Oenothera is being continued. Much of our cytological work on many genera of plants has a very definite bearing on phylogeny and speciation.

Hall, E.R. [E. Raymond - AMS 7: 721]

Projects under way at the Museum of Vertebrate Zoology of possible interest to the Association for the Study of Speciation follow:

- A monographic study of the American weazels, ¾ toward completion, by E. Raymond Hall.
- A systematic and natural history account of the American pocket mice, Genus *Perognathus*, ½ toward completion, by Seth B. Benson.
- A systematic and distributional study of the juncos of North America, in press, by Alden H. Miller.
- Morphological responses to ecological factors as revealed by mammals through San Gorgonio Pass, California, ½ toward completion, by Floyd E. Durham.
- Ecology and systematics of California chipmunks (*Eutamius*), 9/10 toward completion, by David H. Johnson.
- Race formation in song sparrows in San Francisco Bay area, just begun, by Joe T. Marshall.
- Analysis of variation in *Uta stansburiana*, ½ toward completion, by Daniel F. Tillotson.

Hovanitz, W. [William - AMS 8: 1182]

Work is being done now on the geographical color variation in butterflies as related to the ecological conditions of the habitat. A physiological study of the different color races is being attempted in order to obtain information which may lead to a physiological understanding of parallel variation among phylogenetically unrelated animals.

Hubbell, T.H. [Theodore Huntington - AMS 7: 860]

Work on speciation in progress includes: Studies of the short-winged grasshoppers of the genus *Aptenopedes* and of the Puer Group of the genus *Melanoplus* in Florida and the southeastern coastal plain, with reference to their differentiation with relation to present and past isolating factors. This is a continuation and expansion of the work reported on in [a] 1932 paper (Misc. Pub. Mus. Zool. Univ. Mich., 23).

Further work is in progress on the cave- and camel-crickets of North America, especially on the basis of new materials from Mexico and Alabama, and from pocket-gopher burrows in Florida and New Mexico, which has important bearing on the conclusions as to phylogeny and racial differentiation arrived at in a 1936 paper (Univ. Florida Pub., Biol. Ser., 2). [7|8]

Jones, D.F. [Donald Forsha - AMS 7: 924]

Lewis M. Roberts and D.F. Jones are making a study of induced chromosome rearrangements in long inbred strains of maize. This unusual material makes possible the detection and study of minute changes in morphological characters.

King, W. [Willis - AMS 7: 972]

I hope to do some work on the development of the species (or subspecies) of Necturus in the

Tennessee Valley, within the next year.

Miller, A.H. [Alden Holmes - AMS 7: 1217]

A manuscript of 450 pages, now in press, entitled "Speciation in the avian genus Junco," deals with an analysis of hybrids and intergradational complexes between species and races of a number of forms of this genus, and a survey of gradients, or "clines," and of distribution throughout the genus.

Montgomery, B.E. [Basil Elwood - AMS 7: 1241]

I have recently encountered some problems in speciation in connection with a study of the Odonata of South Carolina. Intermediate forms related to northern and southern species appear to have been found here. I am assembling further material for a study of some of these groups.

Netting, M.G. [Morris Graham - AMS 7: 1296]

I have limited my studies geographically to the eastern United States. Within this area I am interested in every factor, both past and present, which may throw additional light upon the origin of herpetological species and subspecies. The various lines of attack which I am following at present are:

- Variation in scutellation - detailed scale counts of snakes to provide the range of variation in laboratory hatchlings or newly born young of a given species from a given area, range of field-caught juveniles, and range of field-caught adults to determine what environmental selection occurs. As this work progresses an attempt will be made to compare the various ranges of geographically different series. Existing published tabulations are of little value since few workers have separated juveniles, which display wide variation, from adults, which have much narrower ranges.
- Relative growth - in eastern salamanders, especially Plethodons, I have measured extensive series and determined that in several species at least the growth rate of the tail is much more rapid than that of the body, and that the gross appearance of the adult animal is much affected, therefore, by the maximum body size attained. Thus, Plethodon nettingi anf [sic] P. welleri, mountain-top endemics which never attain large size, have short tails although they have been derived from a lowland stock with tails which are even longer than their bodies.
- Geographic form gradients - certain external features of frogs are known to show roughly N to S or E to W trends. [8|9] I am engaged in critical examinations of large series of wide-ranging eastern species in an attempt to list all such geographic (or possibly edaphic) modifications of form. For example, in several eastern U.S. frogs, New England specimens have short, obtuse snouts, large feet, and blunt toes whereas Florida specimens have acuminate snouts, small feet, and sharply pointed toes.
- Montane [Mountain?] speciation - faunal studies of the numerous Appalachian knobs and peaks for the purpose of determining the spruce-fir herpetofauna of each, and the relative numbers of individuals of the various species on each peak. Publications include:
  Netting, M. Graham, & M. B. Mittleman. 1938. Description of *Plethodon richmondi*, a new salamander from West Virginia and Ohio. Ann. Carnegie Mus., 27: 287-293. Pl. 30.
  Green, N. Bayard. 1938. A new salamander, *Plethodon nettingi*, from West Virginia. Ann. Carnegie Mus., 27: 295-299.

Rich, W.H. [Willis Horton - AMS 7: 1471]

My interest in this field arises chiefly from my work with the Pacific Salmon of which distinct populations exist in separate streams and even in the separate tributaries of a large river. Some of these "races" are indistinguishable on any morphological basis but are separated on account of the strong tendency for each fish to return to its "home stream." Others of these races may be distinguished by morphological characters that appear to be genetic. Still others are distinguished by characters that are almost certainly environmental. I have not published directly in this field but the following citation contains pertinent material:

Rich, W.H. & Holmes, H.B. 1929. Experiments in marking young Chinook salmon on the Columbia River, 1916-1927. Bull. U.S. Bureau of Fisheries, XLIV:1047.

Considerable data has accumulated from other marking experiments than those covered by the above citation and will eventually be reported.

Rogers, J.S. [James Speed - AMS 7: 1502]

About a half dozen zoologists working in Florida have found that our local fauna offers numerous examples of apparently active speciation. Associated with many abrupt changes in soil, drainage, vegetation and topography (small scale but important) there are a host of virgin islands and many interwoven highways and barriers that have produced more or less complete isolation of small populations. That latter show definite qualitative and quantitative differences in characters that enable the worker to recognize from what restricted locality the specimens were taken – often to a particular ravine or isolated "hammock." In some instances all specimens taken from a given ravine show a character that elsewhere in the range occurs perhaps in a very small percentage of any series. [9|10]

Many of the barriers are not absolute but from time to time are briefly erased by climatic extremes. On the other hand many areas are marginal for occupancy, inhabited by a given form for long periods but then sterilized, so to speak, by climatic extremes and then reinvaded with the return of normal conditions.

At present the Orthoptera (Hubbell), pocket gophers (Sherman), the turtles (Carr), wolf spiders (Wallace), crawfishes (Hobbs) and crane-flies (Rogers) all show the phenomena mentioned above quite markedly. Hobbs' doctorate dissertation on the crawfishes, now in process of completion, shows an almost bewildering amount of speciation and the existence of innumerable local populations.

In my own group, the crane-flies, local populations are numerous and striking; breeding studies on crosses between th[e]se have been going on rather intermittently for more than 10 years. Some of Hubbell's work and some of Carr's has been published already.

Rosenblad, L.E. [Lawrence Eric - AMS 8: 2113]

I have just finished my doctorate problem on the comparative morphology of the reproductive tracts of thirty-six Drosophilinae and have given group relationship[s] on the basis (of) my dissections.

Schultz, A.H. [Adolph Hans - AMS 7: 1572]

A detailed study of a large sample of the population of gibbon from one locality is being made.

Sonneborn, T.M. [Tracy Morton - AMS 7: 1672]

Notes concerning current work on physiological isolating mechanisms in Protista: Two types of isolating mechanisms in Protista have been brought to light by recent work. The first involves variations in the mechanisms for bringing cells together in sexual union. This has been most fully investigated by Moewus in the unicellular green alga *Chlamydomonas*. He finds sexual attraction depends upon differences in the relative amounts of cis and trans dimethyl crocetin produced. The proportions produced are genically determined. Cells respond chemotactically to mixtures of the two substances that differ sufficiently from the proportions produced by the cells themselves. Closely related "species" produce the same two "sex stuffs" in the same or different proportions; other "species" of the same genus, less closely related, do not produce the same "sex stuffs" and so are sexually isolated.

Similar systems of interbreeding have been found in several species of *Paramecium* by Jennings, Sonneborn, Gilman and Giese, and in *Euplotes* by Kimball; but in these ciliate Protozoa no chemical analysis of "sex stuffs" has been made. The situation differs in several important respects from that in

the algae; a single taxonomic species consists of three or more groups of races that cannot interbreed, though the races of any one group [10|11] interbreed freely; in at least some species of *Paramecium*, these sexually isolated genetical species are morphologically indistinguishable. At present, there appears to be no broad geographical isolation of the diverse genetical species,- several or all are found in the same region. Presumably more or less diverse "sex stuffs" are involved in the mating reaction in the different genetical species; this is further indicated by characteristic differences in the temperature range and time of day in which the mating reaction may occur. In *Paramecium aurelia*, an evolutionary relationship between the genetical species is indicated by the existence of a weak mating reaction not capable of leading to actual mating between two mating types belonging to different genetical species.

A second isolating mechanism appears in the discovery by Sonneborn of lethal interactions between different genetical species of *Paramecium aurelia*. Various races of one of the three genetical species produce substances lethal to all races of the other two genetical species and even to some races of the same species. At least three such lethal substances produced by different races have been found. These substances operate quickly and must operate to bring about at least local isolation of the races and species producing the lethal substances. In agreement with this, the genetical species producing the lethal substances is by far the commonest type of *Paramecium aurelia* found in nature.

Willey, A. [Arthur - AMS 5: 1211]

Informative note on Wing-variation in *Allocapnia pygmaea*: In a letter to *Nature* for July 17, 1937, I described a forewing of the stonefly, *Allocapnia pygmaea* Burmeister, in which the radial sector and median vein arose by a common stalk, the mediosectoral pedicel, from the arculus, as is the rule in the hindwings of Plecoptera. Two examples of this variant were found in material collected in 1936, one in a right, the other in a left forewing[.] None was found in 1937 and no observations were made in 1938. In the season of 1939 one right forewing bearing the same character was obtained. The expectation was that the concurrent presence of a mediosectoral pedicel in both forewings of one individual must happen occasionally. This expection has been fulfilled in an instance observed during the season now under way (1940). The frequency of the variation thus amounts to five forewings in a total of two thousand which have been examined, or 0.25 percent. The observations have been made in a Laurentian district named Mille Isles near St. Jerome in central Quebec province. [11|12]

Discussion from Members Concerning Statement of the Objectives of the Society for the Study of Speciation

Bartlett, H.H. [Harley Harris - AMS 7: 100]

Since the Evolution and Genetics program of the Am. Soc. Of Naturalists needs to be reaffirmed, I suggest that society as a convenient organization through which to accomplish exactly what you want to do.

Bartsch, P. [Paul - AMS 7: 102]

Considers it a timely undertaking. Removal of fixed ideas and opening up of the question anew.

Blakeslee, A.F. [Albert Francis - AMS 7: 159]

What is the relation of this to the English organization? Organized by J. Huxley, Turrell [sic-Turrill] et al?

Boyden, A. [Alan Arthur - AMS 7: 189]

I do not know how broadly "speciation" is viewed by the Committee. Logically, there should be several people who know what species are to balance those who are trying to find out how they arise. And all this knowledge bears on the general principles of animal "relationship" and plant "relationship" and how modern genetic knowledge bears on both. I hope the Association will not limit itself to population studies, important as they are.

Clausen, R.T. [Robert Theodore - AMS 7: 325]

From a taxonomic standpoint, the Committee might render a great service by working towards a uniformity of practice among botanists and zoologists in the treatment of specific and subspecific groups. Such a project might involve the holding of symposia on species and subspecies and the publication of the papers in the booklet of the association.

Clements, F.E. [Frederic Edward - AMS 7: 327]

Possibly scientific visits are a part of the answer. We have found it a handicap that the Alpine Laboratory is not on a trunk line.

Cross, J.C. [James Cecil - AMS 7: 387]

Giving recognition to men who are doing fine work, but do not have facilities or time to make themselves famous. These men have much to contribute, but receive little encouragement to make contributions.

Cushing, Jr., J.E. [John Eldridge]

Would it be feasible to use the publication as a means of exchanging ideas and points of view that ordinarily would not be published yet may be of general interest and which could thus also receive the critical comments of workers in other fields? Controversial questions could conceivably be debated in this way. [12|13]

Danforth, C.H. [Charles Haskell - AMS 7: 406]

The group could be most effective by means of a rather loose organization which would function chiefly through the sponsoring of reviews by competent persons who would bring some phase of the subject up to date at intervals supplying a bibliography of papers in the field since the last review.

Davenport, C.B. [Charles Benedict - AMS 7: 411]

Enhance cooperation of students of geographic distribution and comparative ecology perhaps "collectors" representing museums, herbaria, and introducers of new species for [United States Department of Agriculture], explorers, and students of species (including Homo) in the field.

Davis, W.B. [William B. - AMS 7: 422]

I would like to see this association publish *all* new descriptions of races, species, etc. A centralized place for taxonomic and systematic papers would save hours of hunting up obscure groups.

Ferris, G.F. [Gordon Floyd - AMS 7: 557]

I would suggest that the general plan should especially encourage active local groups composed of men from a wide range of contributing subjects, such as the San Francisco Bay Region group, which contains workers from mammalogy, icthyology, entomology, cytogenetics, paleobotany and several other fields. It is the exchange of views that makes such a group of special significance.

Frizzell, D.L. [Donald Leslie - AMS 7: 608]

To those of us in isolated places, it would be of real help if it were possible to circulate lists of current references on speciation – particularly if the references were accompanied by *very brief* summaries, perhaps a sentence or two for each reference.

Graham, E.H. [Edward Harrison - AMS 7: 677]

Suggested that groups meeting locally and fostered by the organization are most helpful.

A knowledge of seminar groups meeting on the subject would be of interest, and a sponsorship of these groups in a very informal way might serve a purpose.

Huntsman, A.G. [Archibald Gowanlock - AMS 7: 876]

A clear and simple formulation of the problem should be primary. The most suitable cases for particular aspects of the problem should be sought. Very critical consideration of these cases should be encouraged. This might lead to their intensive study.

I am particularly concerned with trying to distinguish between genetic and environmental influences in determining the characters of natural populations. Will this receive particular consideration? [13|14]

James, M.T. [Maurice Theodore - AMS 7: 899]

Perhaps cooperation between specialists in different fields could be of value. I would like to obtain cooperation of a cytogeneticist in my studies of speciation in some Dipterous genera, in which morphological and ecological evidence point to some good problems.

Jenkins, J.A. [James A. - AMS 7: 905]

As a preliminary step the association might serve as a clearing-house for information and to bring

together individuals and groups now scattered throughout the country.

Kofoid, C.A. [Charles Atwood - AMS 7: 990]

A basic discussion of a nomenclature for ultra-specific entities.

Linsdale, Jean Myron [Jean Myron - AMS 7: 1072]

Because of the great deviations in interpretation of geographic variation, even among workers dealing with closely related kinds of organisms, it seems that to summarize and define ideas concerning the smaller taxonomic units of the various groups of plants and animals now being studied in this country would be a valuable undertaking.

Macfarlane, John Muirhead [John Muirhead - AMS 5: 715]

Secure close cooperation and common enthusiasm between workers in this country as well as abroad.

Mickey, G.H. [George Henry - AMS 7: 1214]

The idea probably originated as a result of similar cooperative activities of the Maize geneticists and Drosophila geneticists (such as D.I.S.). I think the general plan of D.I.S. is excellent and could be adapted to the needs of the proposed organization.

Moore, John Percy [John Percy - AMS 7: 1246]

I like the plan of an informal association rather than a fully organized society. Biology in this country is already too formalized.

Myers, G. S. [George Sprague AMS 7: 1281]

It is my opinion that sight has been lost of the fact that other problems beside the central one of speciation should come within the purview of such an organization. The British "Association for the Study of Systematics in Relation to General Biology" seems to me to come nearer the ideal of what an organization of the present type should be.

The limitation of the interest of the new organization to "speciation" is unfortunate. Although speciation (sensu stricto) is the prime problem of systematics, and one of the biggest in biology, it can scarcely be divorced from studies of other phases of the biology of natural populations, such as evolution beyond the mere point of specific differentiation. The field covered by the British organization seems to be much more nearly what is needed. [14|15]

I therefore urge that the field be enlarged to include at least those phases of "speciation" relating the origin and relationship of populations beyond *specific* evolution, and preferably those other fields in which taxonomy so strongly impinges on other biological fields.

To my mind, systematics may be defined as study of the nature and evolution of natural populations of living organisms. Into such a study goes all that can be learned of the morphology, physiology, genetics, paleontology, biogeography, habits and ecology of such populations. To stop short at species seems short-sighted. On just one side, biogeography for example, a society such as that planned could do an enormous amount of good. Of course, it may be argued that a biogeographical society could be organized, but societies are too numerous nowadays, and one could cover all subjects related to the study of natural populations with no loss of efficiency. I rather imagine that were Prof. Huxley available, he would agree that it was this type of society which he envisioned.

Riley, H.P. [Herbert Parkes - AMS 7: 1482]

A plan by which seeds and other material could be exchanged among members in different regions would be beneficial. A sort of a "clearing-house" for the exchange of such material.

Schrader, F. [Franz - AMS 7: 1568]

[Emerson's excerpting] …wouldn't it be better to consider the new association as a joint sub-section of the Soc. of Zoology and Soc. of Botany? This would avoid a lot of the machinery of an independent new society.

Schultz, A.H. [Adolph Hans - AMS 7: 1572]

Form committee or symposium for standardization of terms "race, species, etc.," as applied to fossil man, modern man and other mammals. Encourage mammalian taxonomic studies based upon anatomical characters besides those appertaining merely to skin and skull and supported by statistically adequate series. Study of effect of selection upon correlated variations.

Stickel, W.H. [William Hanson - AMS 8: 2399]

As one part of the program I would suggest that critiques be published of work that should have supplied information on speciation, but which due to the treatment of the subject resulted in little or none.

Taverner, P.A. [Percy Algernon - AMS 7: 1758]

Of course the subject of the dynamics of the origin of species has been in all minds and has been talked about and around ever since the Origin of Species with little result. Whether we have arrived at a point from which any material progress can be made is the question but certainly little can be hoped for if each little specialty remains within its own water-tight compartment. I think important light may be thrown upon this fundamental problem if the geneticist and the systematist get together and supply each to the other what the other needs. I have become more and more aware that many of our subspecific refinements must be considered genetically while geneticists must pay more attention to what [15|16] takes place outside their laboratories. The work done on the effect of various rays upon the modification of genes seems suggestive of some of the possible causes of individual variation and consequently of speciation but the species specialist and genetic experimenter must be brought into collaboration to synthesize results. The questions seem to be, – What causes individual variation? What sifts out some perpetuation, and what produces genetic isolation? If there is any chance of answering these questions I am all for it.

There are several cases in American birds in which the genetic inter-racial interest is large, – The Red-tailed Hawk, *Buteo borealis*, the Great Horned Owl, *Bubo virginianus* and the two Flickers, *Colaptes auratus* and *cafer*. There are others of course but these are the ones that have been brought more particularly to my attention.

Torre-Bueno, J.R. de la.

…I should say as a start to endeavor to get a general consensus of lines of cooperative study and at a later date, try to clear up the very ambiguous status of species. Are they actual biological static entities? Are they purely subjective concepts changing from student to student? Are they permanent or impermanent states? I believe these basic problems should be considered in endeavoring to arrive at something concrete, comprehensible, and divorced from metaphysical subtleties. I further suggest extensive field studies, such as Dr. Kinsey's, on widespread, common and readily observed insects, on a purely objective basis.

Usinger, R.L. [Robert Leslie - AMS 7: 1824]

It would be desirable for the Assn. to actively select profitable lines of research of groups of animals or plants best suited to experimentation and then support or actively promote research along these lines.

Weatherwax, P. [Paul - AMS 7: 1885]

The Maize Genetics Cooperative, Cornell University, has been doing something of the kind in the past several years. They might have some suggestions as to organization and procedure.

Willey, A. [Arthur - AMS 5: 1211]

Moderate life membership fee or option of subscribing for a period of five years.

Woodson, Jr., R.E. [Robert Everard - AMS 7: 1978]

I hope that the new organization will not forget that professional taxonomists have something to offer the ecologist, geneticist, and cytologist, at least in the question of speciation. [16|17]

A Critical Review of *The New Systematics* Edited By Julian Huxley. 1940. Oxford. $6.00 By Alfred E. Emerson

This book is a highly important compilation of studies and viewpoints concerning speciation. Julian Huxley has given a balanced digest of the salient points in the book in his introductory chapter. W.B. Turrill gives an interesting account of recent experimental work with natural and artificial populations and reviews the concepts of "ecotype"[,] "ecospecies", "coenespecies" and "ecological clines". He shows that sound taxonomic, phylogenetic, and geographic conclusions must be based upon a synthesis of methods from various fields. N. W. Timofeeff-Resshovsky has written what the reviewer considers an outstanding summary of "Mutations and Geographical Variation." He gives numerous examples of the distribution of genetic characters in natural species, the distribution of populations in relation to ecological factors, survival experiments of natural populations in relation to ecological factors, various types of isolating mechanisms and a series of mature conclusions concerning important speciation principles. C.D. Darlington has reviewed the cytological and genetic attributes of taxonomic species and discusses the origin of sterility, genetic isolation and hybridity. He shows how complex the concept of the species is from a genetic and cytological aspect. Sewall Wright has summarized his theoretical work on "mutation-pressure", selection-pressure," "inbreeding," "population size," "migration-pressure," "isolation" and their mathematical relationships. He concludes that evolution has not proceeded similarly in all groups but that various factors, quantitatively different, produce various types of results in species evolution. H.J. Muller has written a long and excellent chapter on the relation of the study of Drosophila to systematics. An important phase of Muller's review is the conclusion that "a long period of non-mixing of two groups is inevitably attended by the origination of actual immiscibility, i.e. genetic isolation." Lancelot Hogben, in his chapter on "Problems of the Origin of Species," discusses the complexity of the subject, the role of different types of isolation and their relation to genetic and ecological data. E.B. Worthington deals with geographical distribution of fresh water fishes and gives very interesting data on speciation in relation to ecological factors. C. Diver reviews some cases of closely related species living in the same area and postulates the most probable general cause for the origin of such groups is through random differentiation in small partially isolated populations with little effect of speciation. E.J. Salisbury, writing on "Ecological Aspects of Plant Taxonomy" shows the relation of taxonomic species to ecological conditions and also shows how physiological and ecological characteristics are often of great importance in handling taxonomic problems. W.H. Thorpe, in his chapter on "Ecology and the Future of Systematics," reviews interesting cases where groups without easily detected morphological

characteristics seem to be distinct species and must be detected and studied through various techniques sometimes neglected by the taxonomist. He advocates the adoption of new methods by the taxonomists. G.R. de Beer reviews [17|18] "Embryology and Taxonomy" and discusses the relation of various developmental principles to systematics. He shows how embryology is connected with the taxonomy of young stages of organisms, threshold effects, caeonogenesis, recapitulation (which he discredits in the strict Haeckelian sense), 'gerontomorphosis', 'paedomorphosis', and 'allometry'. W.J. Arkell and J. A. Moy-Thomas give some interesting examples of morphological variation in relation to palaeontological sequences and indicate the difficulty of harmonizing morphological groups with phylogenetic relationships. J. Ramsbottom presents data on the relation of life cycles of fungi to taxonomic systems and shows the difficulty of working out a satisfactory taxonomy among forms which pass through complex stages, in many cases with insufficient knowledge of the life cycle. The existence of strains and various complexities of the reproductive process produce many problems in taxonomy. T. A. Sprague discusses "Taxonomic Botany, with Special Reference to the Angiosperms". In the reviewer's opinion, he presents a very well balanced review of the correlation between taxonomy and phylogeny and the necessity for considering large numbers of characters. W.T. Calman gives a brief statement of "A Museum Zoologist's View of Taxonomy," emphasizing the correlation between taxonomic characters, distribution, life histories and ecological adjustments. J.S.L. Gilmour deals with one of the most interesting and important subjects, "Taxonomy and Philosophy." A critical discussion of certain points in this chapter will be discussed later. John Smart emphasizes the problems involved in the mass of taxonomic material among insects. He shows that the vast number of insect species literally swamp the fow [sic-few] taxonomists now supported by society and makes a plea for the support of greater numbers and a more constructive attitude toward taxonomy on the part of academic institutions. E.B. Ford has written an interesting chapter dealing mainly with the variation and correlated population relations of mimetic butterflies. H.H. Allan deals with "Natural Hybridization in Relation to Taxonomy" and gives some interesting cases of hybrid populations among plants. M.B. Crane discusses "The Origin and Behaviour of Cultivated Plants" and gives many results of artificial selection and hybridization of interest to the student of both natural and cultivated plants. N.I. Vavilov has written the final chapter on "The New Systematics of Cultivated Plants." He particularly refers to the ecological distribution of cultivated plants and their varieties and pleas for international cooperation among students in various biological fields in furthering a knowledge of the plant and animal resources of the world.

As one might expect from a group of such able investigators stimulated by a man with such a broad perspective as Julian Huxley, this work is an important contribution to the difficult task of welding many techniques of investigation pertaining to the central problem of evolutionary dynamics. The reviewer has much more to praise in the volume than to criticize. However, it would seem to be a more useful procedure if some emphasis is placed upon the controversial and questionable conclusions in the book. I have therefore selected a few of the outstanding viewpoints with which I differ for critical discussion in the hope that others will be stimulated to bring new evidence to bear upon these unsettled problems. [18|19]

Nearly every one of the authors in the book has defined the species or has discussed the attributes of the species concept. However, there is marked divergence in the definitions. Some emphasize the morphological distinctions (pp. 62, 91, 306, 395). Others emphasize the genetic characteristics (pp. 11, 62, 147, 159, 161, 252). Almost all the authors emphasize some form of isolation of the populations as a characteristic of species. Most of the authors consider the species to be a real natural biological unit, although some are inclined to bring in such words as 'convenience' and 'arbitrary' into the discussion of the species concept, as if the concept was more the product of the mind of man than an objective entity in nature. In the reviewer's opinion, the most vague definition in the book is that of Gilmour's (p. 468) who states, "A species is a group of individuals which, in the sum total of their attributes, resemble each other to a degree usually accepted as specific, the exact degree being ultimately determined by the more or less arbitrary judgment of taxonomists." I find such a definition an amusing contrast to that recently given by R.W. Wilhelmi – "'Species' of helminths may be defined tentatively as a group of organisms the lipid-free antigen of which, when diluted to 1:4000 or more, yields a positive precipitin test within one hour with a rabbit antiserum produced by injecting 40mg of dry-weight, lipid-free antigenic material and withdrawn ten to twelve days after the last of four intravenous

injections administered every third day."

In an effort to bring together the essential attributes generally included by students of species in the briefest form I define a species as an evolved genetically distinctive reproductively isolated natural population. All criteria are necessary and lack of any one places the group outside of the species category. The species is real in the usual scientific sense. It is believed that such entities exist in nature outside of the human mind and are not merely arbitrary, intuitive or convenient methods of pigeon-holing natural phenomena. In fact, I think it is not too much to say that the species is a prime biological unit taking rank with such concepts as the cell and the organism in practical and theoretical importance.

It would seem unnecessary to argue about the dynamic qualities of the species. The species concept had much to do with the discovery and elaboration of the evolutionary principle and has been amply demonstrated to conform to this principle. Inasmuch as the species is a stage in the evolutionary process, we should expect to find stages leading up to the species in some instances. Such discoveries help in understanding the essential processes of speciation, and help to clarity the dynamic nature of the species. Occasionally a fairly complete series in time may be discovered in contiguous strata by the paleontologist. If the genetically distinctive populations have gradually evolved into genetically distinctive derived populations, reproductively isolated in time, but with no marked point of sharp change, the borderline between species may be difficult to draw and a certain arbitrariness in nomenclature may result (Diver p. 305). [19|20]

This is to be expected and cases should be sought. The term 'lineage' or 'chronological cline' may be used for the whole assemblage. Such discoveries do not invalidate the species concept any more than mitosis invalidates the cell concept or embryology invalidates the organismic concept. In all cases the dynamics should be understood as far as possible and incorporated into the concept.

Morphology is the usual criterion for recognizing genetic distinction of species and it is to be expected that the vast majority of species will show morphological characters that may be used for description and classification. However, the morphological characters are valid for species recognition only when correlated with the genetic constitution of the population. Morphological distinctions based upon physiological or ecological influences without genetic differences are not generally recognized by taxonomists for specific demarkation [sic] and when used because of ignorance of the causes are generally relegated to synonymy when the data are more complete. If the morphology is associated with genetics (sex, phases, etc.) but the population is interbreeding, species are not recognized. Degrees of morphological divergence have been used as criteria (Ginsburg) but in my opinion are not necessarily valid. It is well known that one gene change may cause marked morphological difference while numerous gene changes may result in very slight morphological change. Degree or [sic-of] morphological divergence is often an indication of genetic divergence, however, particularly if complex multiple-factor genetic characters are used. It has also been established that morphological differences within the species are often greater than the morphological differences between species (polymorphism, metamorphosis, cyclomorphosis, as well as variation statistics).

Thus genetic distinction (including gene and chromosome differences) is the fundamental criterion of species distinction and morphological distinction is used only as a means of recognizing the differences in the genetic system. Also other influences of the genetic complex such as physiological, ecological and behavioristic characters may be used for specific recognition if correlated with the genetics of the population. In a few rare instances, these characters are the only ones known, the morphological criteria being absent or unknown (Thorpe).

Reproduction isolation includes all the isolating mechanisms which prevent interbreeding. These may be geographical barriers, ecological barriers, seasonal barriers, time barriers, behavior barriers, and various physiological barriers such as infertility, inviability, and hybrid sterility. Whatever prevents the interbreeding of natural populations regardless of the nature of the barrier, may be considered as a mechanism of reproductive isolation. If hybrids are eliminated through selection or inviability, the populations may be considered as reproductively isolated. The physiological impossibility of intercrossing is of course an established isolating mechanism, but other types of isolation such as discontinuity of ranges may be quite as

effective in preventing interbreeding. Species are known which are the result of hybridization between two species that were once isolated, but remained or became physiologically, ecologically, geographically, cytologically or genetically compatible. [20|21]

The tree of life may therefore sometimes have its twigs joined near the tip. However, all isolating mechanisms if continued ultimately result in intersterility (Muller), so that only rather closely related species and occasionally genera may be expected to produce hybrid species. Occassionally 'hybrid swarms' produce complex populations which present difficulties to the taxonomist (Crataegus and Galapagos finches). These, however, do not invalidate the species concept but add knowledge of the isolating mechanisms involved in speciation.

Asexual and parthenogenetic individuals are reproductively isolated from the outset if no descendent is capable of sexual reproduction. Some authors (Dobzhansky) would confine the species concept to the sexually reproducing populations. Often, however, natural populations not showing genetic differences are stable enough to make specific distinction as practical as in sexually reproducing species (flagellates in the hind gut of termites). In other cases such as bacterial strains and protozoan strains, the species concept as here defined is not strictly applicable. Even though the species concept may ultimately be confined to sexually reproducing populations, it will still remain a prime biological unit much as the cell remains a prime biological unit even though bacteria and protozoan [sic] are considered by some to be acellular.

The species concept is confined to natural populations, rather than to individuals or to artificial populations. This conforms to general practice and is valid because we are primarily concerned with units resulting from natural forces unmodified by experimental man. The population is certainly the unit of the taxonomist and the genetic and reproductive continuity of the population makes it a natural objective entity. A species may be named from a single specimen, but only on the hypothesis that this specimen is a sample of an evolved natural population, genetically distinct and reproductively isolated.

Domestic varieties resulting from artificial selection and isolation are usually not given specific names. If the taxonomist needs to include the domestic variations within his concept, he may refer to the whole assemblage under the term 'coenospecies'. There are instances in which the domestic population derived either through hybridization or from single wild species has become so thoroughly isolated from its wild relatives (which may even be extinct) that the species concept may be applied without much confusion. Man, in other words, may be considered as a natural rather than an artificial influence in such cases.

A subspecies may be defined in the same way that we define the species, except that the reproductive isolation is partial and not complete. I do not think that the subspecies should be confined to a restricted set of factors, ecological or otherwise (Dice). Less sharp barriers to the genetic mixture of the distinctive populations may indicate 'races' or be considered under 'intra-group clines' (Huxley).

Several cases presenting certain difficulties in the application of the species concept are discussed in the book. [21|22] The 'races' of *Drosophila pseudo-obscura* (pp. 5, 24, 253, 356) should be raised to full specific rank according to my definition of a species, even if they cannot be distinguished from pinned specimens alone. A[n]other case where subgroups should be raised to full specific rank are the 'races' of *Anopheles maculipennis* discussed by Thorpe (p. 351). M. Bates (1940 Ann. Ent. Soc. Amer. 33: 343-356) has already made these necessary changes in nomenclature and has reviewed the data. Thorpe (p. 347) misinterprets the case of 'physiological species' in the termite genus *Nasutitermes* which I reported in 1935 (Ann. Ent. Soc. Amer. 28: 369-395). He refers to the case as if one species were involved when two species were actually involved, *N. guayanae* and *N. similis*. He also states, "Although the evidence is not conclusive, this difference (termitophile distribution) is regarded by the * author as evidence of subtle biological differences between the colonies of termites." [This "*" here may refer to a footnote that Emerson omitted in the typing of the *News Bulletin*]. Why is the evidence not conclusive? The statistical significance of the correlated data is quite adequate. Biological assays in other fields are considered conclusive evidence. Of course, further verification with other evidence would help to make the conclusions more valid, but the correlated evidence fits the theory quite adequately. Thorpe states (p. 343) that "minute structural characters will have to be treated statistically and

tests for correlation undertaken." Also he states (p. 343) that "the crux of the whole matter is that the present methods of taxonomic investigation often fail to separate groups which biologically and ecologically are clearly distinct, and which on every biological ground should be classified as distinct species." I should agree with these last statements, but why should Thorpe question the validity of the *Nasutitermes* species after having made these statements? It seems to me that my case is a good example to substantiate Thorpe's general view.

The authors differ somewhat among themselves in the different chapters of the book which touch upon the question of the higher taxonomic categories. Some express the viewpoint that the species is a more distinct and valid unit than the genus or other higher category (pp. 4, 356). Others indicate that certain evolutionary phenomena are better understood in terms of the higher categories than when studied at the level of the species (pp. 356, 365, 438, 458). Arkell and Moy-Thomas (p. 406) feel that higher categories should fit into a uniform scale of values and register a protest against splitting old genera into many genera. Also Huxley (p. 26) and Thorpe (p. 357) protest against the concept of the monotypic genus.

It is the reviewer's opinion that genera and higher categories are more than artificial conventions. They are best understood as groups of species with a high proportion of common heredity due to common ancestry and thus share homologous characters. The evidence points to the reality of the shared genetic complex. The 'phylogenetic tree' is still a convenient symbol for expressing major aspects of these relationships. Genera and higher categories not only show relationship through many correlated morphological characters, but show correlated ecological, geographical, physiological, behavioristic, developmental, and paleontological characters. These characters are usually more stable in time than the characters differentiating species. [22|23] They are also most probably multiple factor characters with a complex genetic basis which may also be coordinated with the development of many other fundamental characters in the organism. Both adaptive and non-adaptive characters are found at all taxonomic levels. Adaptation becomes more obvious as one examines the differences between the higher categories and the higher the category, the more obvious is the adaptation to both ecological and internal functions. However, as adaptation is often associated with convergence due to selection of different germinal stocks, non-adaptive characters may often be used with greater conviction in the detection of phylogenetic relationships.

As more information concerning the characters of species and groups is gathered, and as more species are discovered, relationships become better known, phylogenies become more clear, and evolutionary trends become better understood and their causes established. Nomenclature must keep abreast of this increasing knowledge. Thus old genera are continually being subdivided. A Linnaean genus, even though at times it may still be regarded as a natural group, often becomes a family, an order, or a group of orders as the new species are arranged in their phylogenetic systems. Of course, new evidence often points to polyphyletic groups and characters originally regarded as homologous are seen to be convergent adaptations to similar environments. In such cases, the nomenclature must be revised to fit the more clearly understood relationships. It is greatly to the credit of taxonomic nomenclature that it can be adjusted to the enormous increase in our knowledge of natural populations and their phylogenetic relationships.

In some instances, the taxonomist is probably overimpressed with certain striking characters in the species he is studying and is inclined to translate this impression into his taxonomic system, thus placing closely related species in many monotypic genera. A more balanced judgment based upon many correlated characters is to be recommended in such cases. However, it must also be remembered that through extinction, a species may be left without closely related living species. It may thus, through comparison with the correlated characters of other groups, be placed in a monotypic higher category. I should offer as extreme examples of such monotypic categories the tree *Ginkgo biloba* L., a species belonging to the monotypic order Ginkgoales, and the reptile *Sphenodon punctatus* Gray, a species belonging to the monotypic order Rhynchocephalia. Numerous other examples may be found of valid monotypic families and genera.

It seems to me that the great majority of taxonomists and students of speciation would agree that phylogeny should be the basis of all taxonomic grouping. However, this book contains an attack on this principle which

should not be allowed to go unchallenged. More of the chapters express adherence to the phylogenetic principle (pp. 155, 208, 422, 435, 439, 442, 457, 459). However, Arkell and Moy-Thomas (pp. 395-398, 405) definitely object to making phylogenetic patterns the basis of taxonomy and Gilmour (p. 473) takes the same view. Huxley (pp. 18-20) and Allan (p. 515) discuss the problem. [23|24]

I am surprised that so much controversey [sic] should be waged around the question of the phylogenetic basis of taxonomy. The practice and theory of taxonomy in the fields of botany, zoology and paleontology has had its greatest advance as it became correlated with the theory of evolution. It can not only pursue its future advance by becoming correlated with genetics, cytology, physiology, embryology, and ecology, but its greatest contribution to other fields of inquiry is through such integrations. Lack of clarity in the establishment of the foundation principles hampers this valuable reciprocal development.

There are a number of forces influencing the path of evolution, but they all work on the genetic substrate which bridges the generations. Genetic continuity or phylogeny, therefore, would seem to be the foundation principle for taxonomic substructure. The genetic system is in turn established through autocatalytic continuity, mutation and selection for physiological function and ecological adjustment. Homology rests upon genetic similarities and continuity. Even before the advent of the evolution theory, taxonomists were using homologies as the basis of classification without realizing the full significance of their method or the ultimate meaning of the correlations. Where similarities have been found to be due to analogy, convergence or parallelism, taxonomic revision followed homology and phylogeny. Convergence, due to selection of different genetic systems through common ecological or physiological influences, often give us a clue to the nature and action of evolutionary forces, but taxonomy should, and in the main does, follow the genetic relationships if they differ from the functional relationships. A bat and a bird may be classified together ecologically because of their adaptation for flight, but flight is not used in this case to bring them together in taxonomic classification. One type of classification adds meaning to the other, but confusion of the two in one system would be chaotic[.]

The difficulty in the minds of the taxonomists who advocate dropping the phylogenetic basis of taxonomy seems to result from the misconception that taxonomy should rest upon all attributes of an organism without distinction between analogous and homologous attributes and without necessary correlation of the characters with the genetic system and relationships (Gilmour, p. 472). I would maintain that the future of taxonomy must rest upon clear differentiation between analogous and homologous similarities. Both may occur at all levels of taxonomic grouping, but failure to make the distinction introduces gross errors. Science must aim at the discovery of causative principles and not rest at the point of correlated data. Future discoveries, once the theoretical pattern is well established, are then found to take their proper place in the unified system. This tends to slowly verify the principles and in turn augments our understanding and analysis of the principles. Of course, the principles may be expanded or restricted or even discarded as new data are brought into relationship with the established body of knowledge. However, principles which bring greater order in the total mass of data are not to be discarded for principles which bring less order. [24|25]

Going back to the original case, it seems to me that the principle of genetic continuity of forms brings greater order in the taxonomic system than ecological classification through similar adaptation in polyphyletic groups. Both involve fundamentally important principles, but they may only be harmonized if the genetic relationship is given precedence over the ecological relationship as far as taxonomy is concerned.

All similarities in taxonomic groups, therefore, do not have equal weight. If they are analogues, they add understanding to the system of homologues, but they do not have the same phylogenetic significance nor the same value for taxonomic classification. At the same time they may have great value in demonstrating the selection forces acting upon the genetic systems. Sometimes it is difficult to separate analogues from homologues. The best method is the correlation with the genetic system on the one hand and with the functional adaptive (both internal and external) system on the other hand. The basis of judgment will be a significant series of correlated patterns at various levels of taxonomic relationship which in turn indicates a comparative chronology in the appearance of the characters. When the functional character does not fit into

the genetic pattern of relationship and thus shows polyphyletic origin, taxonomy usually does and should follow the genetic and phylogenetic relationship.

When a series of correlated facts are too meagre to form the basis for a convincing phylogeny, tentative working hypotheses may be substituted temporarily awaiting the accumulation of more data. If the data are so meagre that even a working hypothesis of phylogenetic relationship is not possible, some form of artificial arrangement may be substituted without taxonomic implications such as has recently been suggested by C. Croneis (1938. Utilitarian Classification for Fragmentary Fossils, Jour. Geol. 46:975-984). If other valid biological principles form the pattern of correlation such as factors in the ecological habitat, the classification may follow another nomenclature which runs across but does not interfere or confuse the taxonomic and phylogenetic nomenclature. Polyphyletic groups are thus broken up in taxonomic classification but are often maintained in ecological classification. The causative principles behind correlated facts takes precedence over the technical difficulties of detecting the correlated facts. Our knowledge will probably always be incomplete, but this is no reason for not systematizing it about basic principles derived from correlated data. From such scientific procedure, new correlations as well as new verifications will arise and our self-corrected knowledge will grow and broaden.

Huxley (p. 3) states, "Palaeontology, owing to the nature of its data, can only give us information concerning the course of evolution, and not concerning its mechanism." I would take issue with some of the implications of this statement. Paleontology gives data which refutes or fits theories of causation. Other types of data including experimental data do little more, strictly speaking. [25|26]

Isolation is a causative mechanism of speciation. Paleontology gives much information concerning the manner in which geographical isolation operates. Competition is an important aspect of another causative principle, namely natural selection. Paleontology shows how faunas may develop in the absence of competition from certain forms and then may largely succumb when a highway allows the invasion of a new fauna. It gives direct evidence of stability of types without which theories of the causation of stability would be weaker. Paleontology also gives us the time factor, without which our perspective would be narrower. Often bits of paleontological data beautifully refute theories based upon living specimens only. Fossils may clearly indicate convergence and radiation when the evidence from living forms might be quite confusing. To build up theories of causative mechanisms, large accumulations of data from many fields of inquiry must be correlated and the explanations must be tested in the light of new evidence and controlled experiment when feasible. Palaeontology contributes strikingly to this process of acquiring scientific knowledge and should not be thought of as a purely descriptive science. What an organism is today is not to be explained wholly through the study of the dynamic relations within the organism and between the organism and its present environment. The influence of past forces and conditions have left their imprint on and in the living organism and the palaeontologist gives us many data indicating the action of these past forces.

One of the most interesting phases of the study of speciation is the correlation of ontogeny and phylogeny through embryological comparisons. DeBeer has written a most interesting chapter on this subject and through his other books as well has becomes a leader in this fusion between embryology, genetics and evolution. However, there are some points in his chapter that are not quite consistent. He states (p. 376) that "evolutionary novelties which exert their main effects at later stages of the life-histories (cases of adult variation and 'acceleration') are less likely to produce large changes and may be supposed to give rise to mutants, subspecies, species, and genera; such evolution is characterized by ever-increasing specialization and progressive loss of the potential for further evolution. This process is referred to as 'gerontomorphosis'." Doubtless there is some substantiation of this tendency, but I should think the evolution of mammary glands in mammal, wings in insects, and certain basic sex adaptations would be exceptions to this tendency cited by DeBeer. He states (p. 376) that "It is now clearly recognized that evolutionary novelties may make their appearance in any stage of the life history, and may in subsequent generations become retarded, accelerated, or retain the same position in the time-scale of the ontogeny. In other words, it is just as possible that the adult descendent may resemble the ancestral embryo as that the embryo of the descendent may resemble the ancestral adult." I agree with this statement, but DeBeer seems to take sides instead of giving examples which

would show these tendencies in actual operation. [26|27] On p. 375 he says, "The theory of recapitulation, in this sense of abbreviated and accelerated repitition [sic] of phylogeny, is now discredited and generally abandoned." The solution would seem to be found in the evidence that the organism as a whole including its ontogenetic development and adult integration, is a unit acted upon by selection pressures guiding it to both endo-adaptations and exo-adaptations. Not only this, but ancient selection forces during phylogeny have left their impression upon the present hereditary complex in such a way as to influence recapitulative tendencies, vestigial organs and indirect types of development.

The reviewer would have liked to have seen a more extended discussion of degenerative evolution in the book. This is a neglected aspect of speciation and the data are extremely interesting from a theoretical point of view. The closest approach to this problem is in the chapter by Muller (p. 194) in which he discusses the mechanisms leading to infertility and inviability. Wright (p. 174) touches upon the problem but does not discuss it adequately. Wright does discuss the theory of degenerative evolution in his 1929 paper (Amer. Nat. 63:274-279) and students of speciation will find stimulating ideas in his brief theoretical explanation. I have restated the theory in one of my recent papers (1938, Termite Nests – A Study of the Phylogeny of Behavior, Ecol. Monogr. 8:247-284).

There is a tendency in taxonomy and particularly in the taxonomy of species and subspecies and in genetical analysis to emphasize diversity and discontinuity. Although the mechanisms of change and divergence are of prime importance in understanding many taxonomic phenomena, the mechanisms of stability of type should also receive emphasis and analysis. Although several statements in the book emphasize various aspects of stability, the topic has not been organized and presented adequately.

Another concept that would seem to deserve more idscussion [sic] is 'preadaptation'. I would refer the reader to the recent paper by C. L. Hubbs (1938. Fishes from the caves of Yucatan. Carn. Inst. Washington Publ. No. 491:261-295) for a modern treatment of this problem applied to speciation.

Other topics which might have received more adequate attention would include evolution of asexual populations, evolution of unicellular organisms, and the evolution of integrated population units such as cyclomorphosis, aggregations, sex, family and social evolution. I should also have liked to have seen a fuller treatment of the evolution of interspecific integrated groups such as predator-prey, parasite-host, and symbiotic groupings, although Ford's chapter did deal with some of the interesting data in this field. [27|28]

My last criticism of this book is that the subject index is by no means adequate, a fault that is very serious in view of the use for general reference work that the book deserves.

May I add, however, that if I did not think highly of the book I should not have spent so much time reviewing it, and the portions which I have criticized are by no means the least stimulating. I have gained much in my own perspective and sincerely believe that many other students of speciation will find the book indispensible [sic]. We are very fortunate that it was published before the European war inhibited such fundamental studies. [28|29]

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The mimeographed material here presented to the members of the Society for the Study of Speciation falls short of the plans of the secretary in a number of particulars.

Time from other activities did not permit a more adequate classification or abstracting of the literature which amounted to more than was anticipated. Also there is no claim for completeness in spite of the large number of titles collected. Numerous typographical errors may be found which could not be adequately proof-read in the time available. The bibliography has been mimeographed in such a manner that the items may be cut out and pasted on catalogue cards if desired. The list of members was also made in such a manner that it could be used in place of more expensive addressing systems.

It was deemed best to mimeograph the material rather than to reproduce it by some other process, because the equipment was available to the secretary and the cost was less than would otherwise have been the case.

The society is formed to pass information among the members, but the members should take the responsibility for placing such information in the hands of the secretary in a form which may be easily mimeographed and disseminated. The secretary makes the following suggestions for future mailing:

1. Titles of important publications with appropriate bibliographical citations should be sent. These should include important items missed in former lists.
2. Errors in classification of the literature should be corrected.
3. Suggestions for more useful information should be given, especially suggestions which will save time, energy and expense in the secretary's office and will more adequately serve the members.
4. Critical reviews of important current literature are welcome. It is suggested that the reviews be truly critical rather than informative.
5. Notes concerning fields which deserve special attention of the members together with bibliographies of a special nature should be sent. Views on controversial subjects may be presented through this medium.
6. Suggestions for improvement of the organization and function of the society are welcome. The members should not expect the secretary to personally answer all letters, however, because it is obvious that he cannot enlarge his correspondence to this extent. He will attempt, however, to include important and pertinent suggestions in the mimeographed material for future mailing.

Alfred Emerson, Secretary,
Department of Zoology,
University of Chicago,
Chicago, Ill.

[end of *News Bulletin*]

Address List

Editorial notes for Address List

374 members are listed in the 1941 Address List. Cain has modified names to be as complete as possible. Biographical references [vol.: page] at the end of each entry are: **5**: *American Men of Science* volume 5 (1933); **7**: *AMS* vol. 7 (1944), **8**: *AMS* vol. 8 (1949); **Who**: *Who's Who 1897-1997*. The disciplinary affiliations are self-descriptions, found in these biographical references. Addresses are given as listed in the Address List. Cain's editorial insertations are in [ ]; [n/a] indicates information not available. For brevity, "Department of" has been abbreviated to "Dept." and "University of" to "U.".

Abbe, Ernst Cleveland	Dept. Botany, U. Minnesota, Minneapolis, MN botany [7: 1]
Aldrich, John Warren	Cleveland Museum of Natural History, 2717 Euclid Ave., Cleveland, OH ornithology [7: 17]
Alexander, Edward Gordon	Dept. Biology, U. Colorado, Boulder, CO biology [7: 18]
Allee, Warder Clyde	Dept. Zoology, U. Chicago, Chicago, IL zoology [7: 21]
Anderson, Edgar	Missouri Botanical Garden, St. Louis, MO botany, genetics [7: 34]
Atz, James Wade	New York Aquarium, Battery Park, New York, NY [n/a] [not listed]
Babcock, Ernest Brown	U. California, Berkeley, CA plant genetics [7: 63]
Baerg, William J.	U. Arkansas, Fayetteville, AK entomology [7: 67]
Baier, Jr., Joseph George	623 W. State Street, Milwaukee, WI zoology [7: 68]
Baily, Jr. Joshua Longstreth	4435 Ampudia Street, San Diego, CA biology [7: 72]
Baker, Horace Burrington	Zoological Laboratory, U. Pennsylvania, Philadelphia, PA zoology [7: 76]
Balduf, Walter Valentine	Dept. Entomology, U. Illinois, Urbana, IL entomology [7: 79]
Bamford, Ronald	Dept. Botany, U. Maryland, College Park, MD botany [7: 84]

Banta, Arthur Mangun	Dept. Biology, Brown University, Providence, RI zoology [7: 85]
Bartlett, Harley Harris	Dept. Botany, U. Michigan, Ann Arbor, MI botany [7: 100]
Bartsch, Paul	United States National Museum, Washington, DC biology [7: 102]
Bates, Marston	Rockefeller Foundation, 49 W. 49th Street, New York, NY zoology [7: 106]
Beal, John Mann	Dept. Botany, U. Chicago, Chicago, IL botany [7: 112]
Beasley, J. Otis	Texas Agricultural Experimental Station, College Station, TX cytogenetics [7: 115]
Benedict, Ralph Curtiss	Dept. Biology, Brooklyn, NY botany [7: 128]
Benson, Lyman David	U. Arizona, Tucson, AZ systematic botany [7: 132]
Benson, Seth Bertram	Museum of Vertebrate Zoology, Berkeley, CA vertebrate zoology [7: 133]
Bequaert, Joseph Charles	Harvard Medical School, 25 Shaltuck Street, Boston, MA zoology, botany [7: 134]
Bergner, Anna Dorothy	Carnegie Institution of Washington, Cold Spring Harbor, Long Island, NY genetics, cytology [7: 136]
Berner, Lewis	Dept. Biology, U. Florida, Gainesville, FL entomology [8: 191]
Blackwelder, Richard Eliot	American Museum of Natural History, New York, NY entomology [7: 157]
Blakeslee, Albert Francis	Carnegie Institution of Washington, Cold Spring Harbor, Long Island, NY botany [7: 159]
Blossom, Philip M.	Museum of Zoology, U. Michigan, Ann Arbor, MI zoology [7: 165]
Blum, Harold Francis	3000 39th Street, NW, Washington, DC physiology [7: 166]
Bole, Jr., Benjamin Patterson	Cleveland Museum of Natural History, 2717 Euclid Avenue, Cleveland, OH mammalogy, ecology [7: 172]
Boulton, Wolfrid Rudyerd	Field Museum Natural History, Burnham Park, Chicago, IL ornithology [7: 182]
Bowden, Wray Merrill	Blandy Experimental Farm, U. Virginia, Charlottesville, VA cytogenetics [7: 184]
Boyden, Alan Arthur	Rutgers University, New Brunswick, NJ zoology [7: 189]
Breland, Osmond Philip	Dept. Zoology, U. Texas, Austin, TX zoology, entomology [7: 203]
Brower, Auburn Edmond	5 Hospital Street, Augusta, ME entomology [7: 217]

Brown, Frederick Martin	Fountain Valley School, Colorado Springs, CO physiology [7: 221]
Brues, Charles Thomas	Biological Laboratories, Harvard University, Cambridge, MA zoology [7: 231]
Buchanan, Robert Earle	Iowa State College, 25 Agricultural Hall, Ames, IA bacteriology [7: 236]
Buchholz, John Theodore	U. Illinois, 308 Natural History Building, Urbana, IL botany [7: 236]
Bugbee, Robert Earl	Fort Hays Kansas State College, Hays, Kansas entomology [7: 240]
Bujak, B. J.	Laboratory of Vertebrate Genetics, U. Michigan, Ann Arbor, MI [n/a] [not listed]
Burlingame, Leonas Lancelot	Dept. Biology, Stanford University, Room 426, Jordan Hall, Stanford, CA biology [7: 249]
Burrows, William	Dept. Bacteriology and Parasitology, U. Chicago, Chicago, IL bacteriology [7: 253]
Cain, Stanley Adair	U. Tennessee, Knoxville, TN botany [7: 264]
Calvert, Philip Powell	P.O. Box 14, Cheyney, PA zoology [7: 268]
Camp, Wendell Holmes	New York Botanical Garden, Bronx Park, New York, NY botany [7: 271]
Cantrall, Irving James	Museum of Zoology, Ann Arbor, MI zoology [7: 276]
Cartledge, Joseph Lincoln	West Virginia University, Morgantown, WV botany [7: 288]
Castle, William Ernest	U. California, Hilgard Hall, Berkeley, CA zoology [7: 292]
Chace, Jr., Fenner Albert	Museum Comparative Zoology, Cambridge, MA zoology [7: 296]
Chadwick, Leigh Edward	Pueblo Junior College, Pueblo, CO physiology [7: 296]
Chamberlin, Ralph Vary	U. Utah, Salt Lake City, UT zoology [7: 298]
Chandler, Asa Crawford	Rice Institute, Houston, TX zoology [7: 299]
Chaney, Ralph Workds	U. California, Berkeley, CA geology, paleobotany [7: 301]
Chapin, Edward Albert	United States National Museum, Washington, DC entomology [7: 301]
Chapin, James Paul	Dept. Ornithology, American Museum Natural History, New York, NY ornithology [7: 301]
Chester, Kenneth Starr	Dept. Botany and Plant Pathology, Oklahoma Agricultural and Mechanical College, Stillwater, OK plant pathology [7: 308]
Clark, Frances J.	Dept. Genetics, Connecticut Agricultural Experiment Station, New Haven, CT [n/a] [not listed]
Clark, Hubert Lyman	Museum of Comparative Zoology, Cambridge, MA zoology [7: 320]

Clausen, Jens Christian	Carnegie Institution of Washington, Stanford University, Stanford, CA botany [7: 325]
Clausen, Robert Theodore	Dept. Botany, Cornell University, Ithaca, NY botany [7: 325]
Cleland, Ralph Erskine	Dept. Botany, Indiana University, Bloomington, IN botany, genetics [7: 326]
Clemens, Wibert Arnie	Dept. Zoology, U. British Columbia, Vancouver, British Columbia, Canada biology [7: 326]
Clements, Frederic Edward	Carnegie Institution of Washington, Mission Canyon, Santa Barbara, CA botany [7: 327]
Clench, William James	Museum of Comparative Zoology, Cambridge, MA zoology [7: 327]
Cockerell, Theodore Dru Alison	908 Tenth Street, Boulder, CO zoology [7: 333]
Coker, Robert Ervin	Dept. Zoology, U. North Carolina, Chapel Hill, NC zoology [7: 338]
Cole, Leon Jacob	College of Agriculture, U. Wisconsin, Madison, WI zoology [7: 340]
Cole, Jr., Arthur Charles	Dept. Entomology, U. Tennessee, Knoxville, TN entomology [7: 339]
Collins, Julius Lloyd	Experiment Station, U. Hawaii, Honolulu, HI genetics [7: 344]
Cooper, Kenneth Willard	Dept. Biology, Princeton University, Princeton, NJ zoology [7: 360]
Cort, William Walter	615 N. Wolfe Street, Baltimore, MD helminthology [7: 367]
Costello, Donald Paul	Dept. Zoology, U. North Carolina, Chapel Hill, NC zoology [7: 369]
Coventry, Alan Freeth	U. Toronto, Hart House, Toronto, Ontario zoology [7: 372]
Cowan, Ian McTaggart	Provincial Museum, Victoria, B.C., Canada mammalogy, ornithology [7: 373]
Crampton, Henry Edward	Barnard College, Columbia University, New York, NY zoology, experimental biology [7: 379]
Cross B. or E. C.	Zoology, Royal Ontario Museum, Toronto, Ontario, Canada [n/a] [not listed]
Cross, James Cecil	Texas College of Arts and Industries, Kingsville, TX zoology [7: 387]
Crow, James Franklin	Dept. Zoology, U. Texas, Austin, TX genetics [7: 389]
Culbertson, James Thomas	College of Physicians and Surgeons, Columbia University, 630 W. 168th Street, New York, NY bacteriology [7: 392]
Cumley, Russell Waters	Dept. Genetics, U. Wisconsin, Madison, WI genetics [7: 393]
Curtis, Winterton Conway	College of Arts and Science, U. Missouri, Columbia, MO zoology [7: 399]
Cushing, Jr., John Eldridge	California Institute of Technology, Pasadena, CA [n/a] [not listed]
Dahl, Anthony Orville	Dept. Biology, Harvard University, Cambridge, MA botany [7: 402]

Danforth, Charles Haskell	Dept. Anatomy, Stanford University, Stanford, CA anatomy [7: 406]
Darlington, Jr., Philip Jackson	Museum of Comparative Zoology, Harvard University, Cambridge, MA entomology [7: 409]
Davenport, Charles Benedict	Cold Spring HarborLong Island, NY zoology [7: 411]
Davis, Bradley Moore	Dept. Botany, U. Michigan, Ann Arbor, MI botany [7: 415]
Davis, William B.	Dept. Fish and Game, Box 254 FE, College Station, TX wildlife conservation and management [7: 422]
DeGaris, Charles Francis	Dept. Anatomy, U. Oklahoma School of Medicine, Oklahoma City, OK anatomy, zoology [7: 432]
Deichmann, Elizabeth	Museum of Comparative Zoology, Cambridge, MA zoology [7: 433]
Demerec, Millislav	Dept. Genetics, Carnegie Institution of Washington, Cold Spring Harbor, NY zoology, genetics [7: 436]
Detling, LeRow Ellsworth	Dept. Botany, U. Oregon, Eugene, OR botany [7: 422]
Dice, Lee Raymond	Dept. Zoology, U. Michigan, Ann Arbor, MI zoology [7: 446]
Doak, Clifton Childrews	Dept. Biology, College Station, TX plant morphology [7: 455]
Dobzhansky, Theodosius	California Institute of Technology, Pasadena, CA zoology, biology [7: 457]
Doering, Kathleen Clare	Dept. Entomology, U. Kansas, Lawrence, KS entomology [7: 459]
Dorf, Erling	Dept. Geology, Princeton University, Princeton, NJ geology, paleobotany [7: 464]
Doutt, J. Kenneth	Carnegie Museum, Pittsburgh, PA mammalogy [8: 645]
Downes, John Antony	Dept. Anatomy, The University, Glasgow, Scotland [n/a] [not listed]
Drake, Carl John	Dept. Zoology and Entomology, Iowa State College, Ames, IA entomology [7: 472]
Dreyer, William Albert	Dept. Zoology, U. Cincinnati, Cincinnati, OH zoology [7: 475]
Dunbar, Carl Owen	Peabody Museum, Yale University, New Haven, CT geology [7: 481]
Dunn, Emmett Reid	Dept. Biology, Haverford College, Haverford, PA zoology [7: 485]
Dymond, John Richardson	Royal Ontario Museum of Zoology, Toronto, Ontario, Canada zoology [7: 492]
Eigsti, Orie Jacob	Dept. Botany, U. Oklahoma, Norman, OK botany [7: 507]
Eller, Eugene Rudolph	Dept. Paleontology, Carnegie Museum of Natural History, Pittsburgh, PA paleonology [8: 705]
Elton, Charles	Bureau of Animal Populations, University Museum, Oxford, England ecology [Who]
Emerson, Alfred Edwards	Dept. Zoology, U. Chicago, Chicago, IL zoology [7: 519]

Enders, Robert Kendall	Dept. Zoology, Swarthmore College, Swarthmore, PA biology [7: 522]
Engels, William Louis	Dept. Zoology, U. North Carolina, Chapel Hill, NC zoology [7: 522]
Epling, Carl Clawson	Dept. Botany, U. California, Los Angeles, CA botany [7: 526]
Errington, Paul Lester	Insectary, Iowa State College, Ames, IA vertebrate ecology [7: 528]
Evans, Alice Catherine	Bacteriologist, National Institute of Health, Washington, DC bacteriology [7: 532]
Evans, Gertrude	Beloit CollegeBeloit, WI zoology [7: 533]
Ewan, Joseph Andorfer	Dept. Biology, U. Colorado, Boulder, CO botany [8: 739]
Ferris, Gordon Floyd	Natural History Museum, Stanford University, Stanford, CA zoology [7: 557]
Field, Henry	Field Museum of Natural History, Chicago, IL anthropology [7: 560]
Flory, Jr., Walter S.	Texas Agricultural Experiment Station, College Station, TX genetics, horticulture [7: 578]
Fosberg, Francis Raymond	Dept. Botany, 1101 21st Street S., Arlington, VA botany [7: 588]
Fox, Henry	R.F.D. 1, Cape May Court House, Cape May, NJ biology [7: 593]
Friesner, Ray Clarence	Dept. Botany, Butler University, Indianapolis, IN botany [7: 607]
Frison, Theodore Henry	Natural History Building, Urbana, IL entomology [7: 608]
Frizzell, Donald Leslie	Geologist, International Petroleum Co. Ltd.,Negritos, Peru paleontology [7: 608]
Fulton, Bentley Ball	Dept. Entomology, State College, Raleigh, NC entomology [7: 614]
Gager, Charles Stuart	Brooklyn Botanic Garden, 1000 Washington Ave., Brooklyn, NY botany [5: 393]
Gates, Reginald Ruggles	Kings College London, The Strand, London, England anthropology [7: 630]
Gause, G. F.	Dept. Biology, Malaja Bronnaia 12, kv, 33, Moscow, U.S.S.R. [n/a] [not listed]
Gerould, John Hiram	Dept. Zoology, Dartmouth College, Hanover, NH zoology [7: 639]
Gleason, Henry Allen	New York Botanical Garden, Fordham Station, New York, NY botany [7: 655]
Gloyd, Howard Kay	Chicago Academy of Sciences, Lincoln Park at Clark and Ogden, Chicago, IL zoology [7: 657]

Gluckmann, E.	U. the Witwatersrand, Milner Park, Johannesburg, South Africa [n/a] [not listed]
Goin, Coleman Jett	Dept. Biology, U. Florida, Gainesville, FL herpetology [7: 660]
Goldschmidt, Richard Benedikt	Dept. Zoology, U. California, Berkeley, CA zoology [7: 662]
Goodman, George James	Dept. Botany, Iowa State College, Ames, IA botany [7: 666]
Goodrich, Herbert Baker	Dept. Biology, Wesleyan University, Middleton, CT pharmacology [7: 667]
Goodspeed, Thomas Harper	Dept. Botany, U. California,Berkeley, CA botany [7: 668]
Gordon, Myron	New York Aquarium, New York, NY zoology, genetics, ichthyology [7: 670]
Graham, Edward Harrison	Dept. Biology, 232 Prospect Street, Chevy Chase, MD botany [7: 677]
Graham, Herbert William	Dept. Zoology, Mills College, CA oceanography, zoology [7: 678]
Granovsky, Alexander Anastacievitch	Dept. Entomology, U. Minnesota, St. Paul, MN entomology [7: 680]
Greenway, Jr., James Cowan	Museum of Comparative Zoology, Cambridge, MA zoology [7: 693]
Gregor, J. L.	Scottish Plant Breeding Station, Corstorphine, Edinburgh, Scotland [n/a] [not listed]
Gregory, Joseph Tracey	Bureau of Economic Geology, U. Texas, Austin, TX vertebrate paleontology [7: 694]
Gregory, William King	American Museum of Natural History, New York, NY vertebrate paleontology [7: 695]
Gustafson, Alton Herman	Dept. Biology, Williams College, Williamstown, MA cytology [7: 709]
Guyer, Michael Frederic	Dept. Zoology, U. Wisconsin, Madison, WI zoology [7: 711]
Hadley, Philip Bardwell	Western Pennsylvania Hospital, Pittsburgh, PA bacteriology [7: 715]
Hall, E. Raymond	Museum of Vertebrate Zoology, U. California, Berkeley, CA vertebrate zoology [7: 721]
Hamburger, Viktor	Dept. Zoology, Washington University, St. Louis, MO zoology [7: 728]
Hamerstrom, Jr., Frederick Nathan	U. Wisconsin, Plainfield, WI wildlife [7: 729]
Hart, John Lawson	Pacific Biological Station, Nanaimo, B.C., Canada fisheries [7: 755]
Hartman, Carl G.	Johns Hopkins University Medical School, Baltimore, MD embryology, physiology [7: 757]
Hatch, Melville Harrison	Dept. Zoology, U. Washington, Seattle, WA zoology, entomology [7: 764]
Helwig, Edwin Royer	Dept. Zoology, U. Pennsylvania, Philadelphia, PA zoology [784]
Henrici, Arthur Trautwein	U. Minnesota, 18 Millard Hall, Minneapolis, MN bacteriology [5: 501]

Hill, John Eric Douglas	American Museum of Natural History, New York, NY zoology [7: 812]
Hobbs, Jr., Horton Holcombe	Dept. Biology, U. Florida, Gainesville, FL zoology [7: 821]
Holton, Charles Stewart	[pathologist] Agricultural Experiment Station, USDA, Pullman, WA plant pathology [7: 837]
Hopkins, Milton	Dept. Botany, U. Oklahoma, Norman, OK botany [7: 842]
Hough, Walter Seneff	Agricultural Experiment Station, Virginia Experiment Station [?] Wincester, VA entomology [7: 849]
Hovanitz, William	California Institute of Technology, Pasadena, CA genetics [8: 1182]
Howell, Alfred Brazier	Dept. Anatomy, Johns Hopkins Medical School, Baltimore, MD anatomy [7: 855]
Howell, Benjamin Franklin	Princeton University, Princeton, NJ geology, invertebrate paleontology [7: 855]
Hubbard, Charles Edward	Herbarium, Royal Botanic Garden, Kew, Surrey, UK botanist [Who]
Hubbell, Theodore Huntington	Dept. Biology, U. Florida, Gainesville, FL entomology [7: 860]
Huestis, Ralph Ruskin	Dept. Zoology, U. Oregon, Eugene, OR zoology [7: 863]
Huff, Clay G.	U. Chicago, Chicago, IL bacteriology, parasitology [7: 863]
Hughes, Roscoe Duvall	Dept. Biology, Medical College Virginia, Richmond, VA zoology [7: 866]
Hungerford, Herbert Barker	Dept. Entomology, U. Kansas, 323 Snow Hall, Lawrence, KS entomology [7: 871]
Huntington, Ellsworth	Yale University, New Haven, CT geology, geography [7: 875]
Huntsman, Archibald Gowanlock	Dept. Biology, U. Toronto, Toronto, Ontario, Canada biology [7: 876]
Huskins, Charles Leonard	Dept. Genetics, McGill University, Montreal, Quebec, Canada genetics, cytology [7: 878]
Huxley, Julian Sorell	Zoological Society of London, Regents Park, London, UK zoology [Who]
Ide, Frederick Palmer	Dept. Biology, U. Toronto, Toronto, Ontario, Canada invertebrate zoology [7: 883]
Irwin, Malcolm Robert	Dept. Genetics, U. Wisconsin, Madison, WI zoology, genetics [7: 889]
Iseley, Frederick B.	Trinity University, Waxahachie, TX zoology [7: 890]
Ives, Philip Truman	Dept. Biology, Amherst College, Amherst, MA genetics [7: 891]
James, Maurice Theodore	Dept. Entomology and Zoology, Colorado State College, Fort Collins, CO entomology [7: 899]
Jellison, William Livingston	Parasitologist, United States Public Health Service, Hamilton, MT

	parasitology [7: 904]
Jenkins, James A.	Dept. Genetics, U. California, 314 Hilgard Hall, Berkeley, CA genetics [7: 905]
Jennings, Herbert Spencer	U. California, Los Angeles, CA zoology, genetics [7: 907]
Jepsen, Glenn Lowell	Dept. Geology, Princeton University, Princeton, NJ vertebrate paleontology [7: 909]
Johnson, Willis Hugh	Dept. Biology, Stanford University, Stanford, CA zoology [7: 920]
Jones, Donald Forsha	Connecticut State Experimental Station, Box 1106, New Haven, CT genetics [7: 924]
Jones, G. Neville	Dept. Botany, U. Illinois, Urbana, IL [n/a] [not listed]
Just, Theodor Karl	Dept. Biology, U. Notre Dame, Notre Dame, IN botany [7: 936]
Keck, David Daniels	Carnegie Institution of Washington, Stanford University, Stanford, CA botany [7: 947]
Kesteven, G. L.	318 Post Office Place, Melbourne, Australia [n/a] [not listed]
Kimball, Richard Fuller	Dept. Zoology, Johns Hopkins University, Baltimore, MD genetics [7: 967]
King, Willis	Great Smoky Mountains National Park, National Park Service, Gatlinburg, TN fisheries management, herpetology [7: 972]
Kinsey, Alfred Charles	Dept. Zoology, Indiana University, Bloomington, IN zoology, entomology [7: 974]
Kirby, Harold	Dept. Zoology, U. California, Berkeley, CA zoology [7: 975]
Klauber, Laurence Monroe	233 W. Juniper Street, San Diego, CA electricity, herpetology [7: 979]
Klots, Alexander Barrett	Dept. Biology, 17 Lexington Ave., New York, NY biology [7: 983]
Knight, Harry Hazelton	Dept. Zoology, Iowa State College, Ames, IA entomology [7: 984]
Knight, James Brookes	Dept. Geology, Princeton University, Princeton, NJ paleontology [7: 984]
Kofoid, Charles Atwood	Dept. Zoology, U. California, Berkeley, CA zoology [7: 990]
Kroeber, Alfred L.	U. California, Berkeley, CA anthropology [7: 1002]
Krogman, Wilton Marion	U. Chicago, Chicago, IL anthropology, anatomy [7: 1003]
Lane, John	Institute of Hygiene Sao Paulo, Caixa 2920, Sao Paulo, Brazil [n/a] [not listed]
Lanham, Urless Norton	Dept. Zoology, U. California, Berkeley, CA zoology [8: 1434]
Lewis, C. Bernard	Institute of Jamaica, Kingston, Jamaica [n/a] [not listed]

Light, Sol Felty	U. California, 4047 Life Sciences Building, Berkeley, CA zoology [7: 1065]
Lindegren, Carl Clarence	Dept. Bacteriology, U. Southern California, Los Angeles, CA genetics [7: 1068]
Lindsey, Arthur Ward	Dept. Zoology, Denison University, Granville, OH biology, entomology [7: 1070]
Lindstrom, Ernst Walter	Dept. Genetics, Iowa State College, Ames, IA botany, genetics [7: 1070]
Linsdale, Jean Myron	U. California, Jamesburg Route, Monterey, CA vertebrate zoology [7: 1072]
Linsley, Earle Gorton	U. California, 112 Agricultural Hall, Berkeley, CA entomology [7: 1072]
List, George Milton	Colorado State College of Agriculture & Mechanics, Fort Collins, CO entomology [7: 1073]
Lowenstam, Heinz Adolf	Illinois State Museum, Springfield, IL invertebrate paleontology, paleoecology [7: 1092]
Lowery, Jr., George Hines	Museum of Zoology, Louisiana State University, University, LA ornithology [8: 1535]
Lutz, Frank Eugene	American Museum of Natural History, W 79th St at Central Park, New York, NY zoology [7: 1101]
Lyon, Jr., Marcus Ward	South Bend Clinic, 214 Laporte Avenue, South Bend, IN zoology, pathology [5: 699]
MacArthur, John Wood	Dept. Biology, U. Toronto, Toronto, Ontario, Canada zoology [7: 1108]
Macfarlane, John Muirhead	U. Pennsylvania, 427 W. Hansberry Street, Germantown, PA botany [5: 715]
MacKay, Donald Copeland Gibson	Dept. Zoology, U. Connecticut, Storrs, CT zoology [7: 1134]
Maguire, Bassett	Dept. Botany, Utah State College, Logan, UT botany [7: 1155]
Maina, B. A.	Dept. Zoology, U. Chicago, Chicago, IL [n/a] [not listed]
Mainland, Gordon Beach	Dept. Zoology, U. Texas, Austin, TX zoology, genetics [7: 1156]
Marsh, Frank Lewis	Dept. Biology, Union College, Lincoln, NE ecology [7: 1170]
Martin, Cecil Percy	Dept. Anatomy, McGill University, Montreal, Quebec, Canada anthropology, anatomy [7: 1174]
Mavor, James Watt	Dept. Biology, Union College, Schenectady, NY zoology [7: 1188]
Mayr, Ernst	Dept. Ornithology, American Museum of Natural History, W 79th Street at Central Park, New York, NY zoology [7: 1192]
McCown, Theodore Doney	Dept. Anthropology, U. California, Berkeley, CA physical

	anthropology, prehistory [7: 1118]
McLean, Richard Alfred	Academy of Natural Sciences, 19th and The Parkway, Philadelphia, PA marine biology [7: 1143]
Melander, Axel Leonard	Dept. Biology, City College of New York, New York, NY biology [7: 1198]
Merrill, Elmer Drew	Arnold Arboretum, Jamaica Plain, Boston, MA botany [7: 1204]
Metcalf, Zeno Payne	Dept. Zoology, State College Station, Raleigh, NC entomology [7: 1208]
Michener, Charles Duncan	U. California, 112 Agricultural Hall, Berkeley, CA systematic entomology, insect morphology [7: 1213]
Mickel, Clarence Eugene	Dept. Entomology, University Farm, U. Minnesota, St. Paul, MN entomology [7: 1214]
Mickey, George Henry	Dept. Zoology, Louisiana State University, University, LA zoology, cytogenetics [7: 1214]
Miller, Alden Holmes	Dept. Zoology, U. California, Berkeley, CA zoology [7: 1217]
Miller, Robert Cunningham	California Academy of Sciences, San Francisco, CA zoology [7: 1225]
Milliron, H. E.	Dept. Entomology, U. Minnesota, St. Paul, MN [n/a] [not listed]
Mills, Harlow Burgess	Dept. Entomology, Montana State College, Bozeman, MT entomology [7: 1229]
Milne, Lorus Johnson	Dept. Biology, Randolph-Macon Woman's College, Lynchburg, VA zoology [7: 1230]
Moffitt [Moffett?], James	California Academy of Sciences, San Francisco, CA limnology, fisheries biology [7: 1237]
Moldenke, Harold Norman	New York Botanical Garden, Bronx Park, Fordham Br. P. O., New York, NY botany [7: 1239]
Montgomery, Basil Elwood	Dept. Entomology, Purdue University Lafayette, IN entomology [7: 1241]
Moore, John Percy	Zoological Laboratory, U. Pennsylvania Philadelphia, PA zoology [7: 1246]
Moore, John Alexander	Dept. Biology, Brooklyn College Brooklyn, NY experimental entomology [7: 1246]
Mossman, Harland Winfield	Dept. Anatomy, U. Wisconsin Madison, WI embryology, anatomy [7: 1264]
Moulton, Dudley	35 Elwood Street, Redwood City, CA entomology [7: 1265]
Muller, Cornelius Herman	Dept. Botany, Bureau of Plant Industry, Washington, DC plant ecology [7: 1271]
Muller, Hermann Hoseph	Institute of Animal Genetics, U. Edinburgh, Edinburgh, Scotland

	biology, genetics [7: 1271]
Munz, Philip Alexander	Dept. Botany, Pomona College, Claremont, CA botany [7: 1275]
Murphy, Robert Cushman	Dept. Ornithology, American Museum of Natural History, New York, NY zoology, ornithology [7: 1277]
Murray, William Donald	Dept. Entomology, U. Minnesota Farm, St. Paul, MN entomology [7: 1279]
Myers, George Sprague	Natural History Museum, Stanford University, Stanford, CA zoology [1281]
Nebel, Bernhard R. G.	Agricultural Experiment StationGeneva, NY cytology [7: 1288]
Necker, Walter Ludwig	Chicago Academy of Science, 2001 N. Clark StreetChicago, IL zoology [7: 1289]
Nelson, Casper Irving	Dept. Bacteriology, North Dakota Agricultural College, Fargo, ND bacteriology [7: 1292]
Netting, Morris Graham	Carnegie Museum of Natural History, Pittsburgh, PA herpetology [7: 1296]
Nice, Margarret Morse	5708 Kenwood Ave., Chicago, IL ornithology, psychology [7: 1301]
Oliver, Clarence Paul	Dept. Zoology, U. Minnesota, Minneapolis, MN genetics [7: 1326]
Painter, Reginald Henry	Dept. Entomology, Kansas State College, Manhattan, KA entomology [7: 1345]
Palmer, Miriam Augusta	Dept. Entomology, Colorado State College, Fort Collins, CO entomology [7: 1347]
Palmer, Theodore Sherman	1939 Baltimore Street, NW, Washington, DC zoology [7: 1348]
Park, Thomas	Dept. Zoology, U. Chicago, Chicago, IL zoology [7: 1351]
Parker, George Howard	Harvard Biological Laboratory, Harvard University, Cambridge, MA zoology [7: 1352]
Parr, Leland Wilbur	Dept. Bacteriology, 1335 H. Street, NW, Washington, DC bacteriology, hygiene [7: 1356]
Parshley, Howard Madison	Dept. Zoology, Smith College, Northampton, MA zoology [7: 1357]
Pate, Vernon Sennock	Dept. Entomology, Cornell University, Comstock Hall, Ithaca, NY entomology [7: 1360]
Patterson, Bryan	Field Museum of Natural History, Chicago, IL vertebrate paleontology [7: 1362]
Patterson, John Thomas	Dept. Zoology, U. Texas, Austin, TX zoology [7: 1362]

Pearse, Arthur Sperry	Dept. Zoology, Duke University, Durham, NC zoology [7: 1368]
Pearson, Jay Frederick Wesley	Dept. Zoology, U. Miami, Coral Gables, FL ecological entomology [7: 1369]
Pennak, Robert William	Dept. Biology, U. Colorado, Boulder, CO zoology [7: 1375]
Pickford, Grace Evelyn	Osborn Zoological Laboratory, Yale University, New Haven, CT zoology [7: 1393]
Pitelka, Frank Alois	Dept. Zoology, U. California, Berkeley, CA zoology [8: 1957]
Plough, Harold Henry	Dept. Biology, Amherst College, Amherst, MA biology [7: 1401]
Pope, Clifford Hillhouse	Field Museum of Natural History, Chicago, IL herpetology, biology [7: 1407]
Poulson, Donald Frederick	Dept. Zoology, Yale University, New Haven, CT genetics [7: 1415]
Preston, Dudley A.	Dept. Plant Pathology and Botany, U. Minnesota, St. Paul, MN botany [8: 1988]
Rahn, Otto	Cornell University, Diary Building, Ithaca, NY bacteriology [7: 1436]
Ramaley, Francis	Dept. Biology, U. Colorado, Boulder, CO botany [5: 908]
Rashevsky, Nicolas	U. Chicago, Chicago, IL mathematic biophysics [7: 1443]
Rau, Philip	549 E. Argonne Drive, Kirkwood, MO entomology [7: 1444]
Raup, Hugh Miller	Dept. Ecology, Arnold Arboretum, Jamaica Plain, Boston, NY botany [7: 1445]
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Editorial notes for Bibliography

This bibliography was compiled by Emerson. It lists 1,250 items. Except for several items from 1938, all references are 1939 or 1940. This bibliography was not intended as a canonical set of readings; rather, the coverage aims for breadth and diversity. Not all articles focus only on speciation.

Emerson classified each citation according to his sense of its relevance for speciation studies. This classification took the form of codes placed at the end of each citation (e.g., IA, IIE). His "key" accompanies this bibliography (see page 103). Emerson also placed record numbers at the end of each citation. Some numbering is out of the citation's strict alphanumeric sequence; these errors are preserved here.

Editorial insertions by Cain are in [ ]. Citations were not verified against original sources. Some obvious misspellings have been corrected, and abbreviations have been expanded. Italicising of generic and species names has been introduced. Transcription was hampered in some cases by poor preservation of the original text available for study. Journal names have been verified against William Allan Smith and Frances Lawrence Kent (eds.). 1952. *World List of Scientific Periodicals Published in the Years 1900 to 1950* (London: Butterworths Scientific Publishers). Some journal titles not found in this reference could not be verified elsewhere. These may be mistakes in data entry by Emerson. The notation [?] reflects unreadable data or data missing in the original bibliography.

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[Emerson's Key to Classification of Citations]

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G. General Papers dealing with many factors

I. Distinctions between species and other categories

- A. Morphological distinctions
  - 1. Embryological distinctions
- B. Cytological distinctions
- C. Genetic distinctions
- D. Physiological distinctions
- E. Psychological (Behavioristic) distinctions
- F. Ecologic distinctions
- G. Geographic distinctions
- H. Stratigraphic (Fossil sequence) distinctions

II. Causes of Variation

- A. Gene Mutation
- B. Recombination
- C. Genome (Chromosome) mutation
- D. Non-Mendelian (cytoplasmic) inheritance
- E. Paedomorphosis. Neoteny
- F. Orthogenesis. Heterogony. Heterochrony. Allometry
- G. Recapitulation. Vestigal Structures. Stability of Type
- H. Lamarckian evolution. The inheritance of acquired somatic characters

III. Isolation

- A. Topographical isolation
- B. Spatial isolation
- C. Time isolation
- D. Ecological isolation
- E. Seasonal isolation
- F. Physiological isolation
  - 1. Behavior (Psychological) isolation
  - 2. Physiological isolation (proper)
  - 3. Mechanical isolation
  - 4. Gamete Sterility (Infertility)
  - 5. Embryological impairment (Inviability)
  - 6. Adult sterility
  - 7. Parthenogenesis. Apomixis
  - 8. Asexual reproduction
  - 9. Population waves
  - 10. Migration pressure

IV. Natural Selection

- A. Overproduction
- B. Competition
- C. Preadaptation
- D. Degenerative evolution
- E. Convergence
- F. New habitats (radiation)
- G. Physical and chemical factor correlation
- H. Biotic factor correlation
- I. Cyclomorphosis
- J. Sexual characters. Sex ratios. Sexual selection
- K. Population units

V. Artificial Selection

Exploring the Borderlands

Documents of the Committee on Common Problems of Genetics, Paleontology, and Systematics, 1943-1944

Joe Cain (editor)
Foreword by Ernst Mayr

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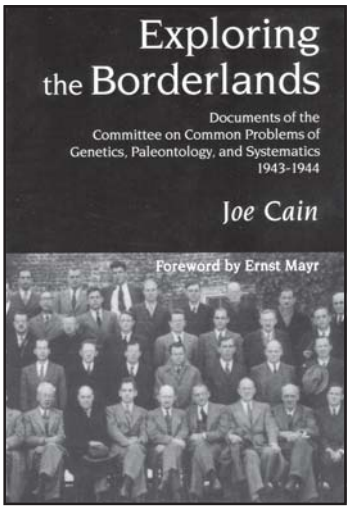

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Organising the Society for the Study of Speciation was a simple affair in 1939. The job of implementing its vision fell upon the entomologist Alfred Emerson, recruited to serve as Secretary. 'The need was felt by many students of speciation for a greater degree of integration between the various fields,' he wrote.

> 'Those contributing to an understanding of the factors influencing speciation are often in fields and institutions which have little direct contact with those who are attacking the problem from somewhat different angles and are using different techniques....The general object of the Society [will be] to institute an informal information service which will tend to correlate the various approaches.'

Emerson posted the Society's first batch of material in March 1941. This featured a 29-page news bulletin, including a specially written review of Julian Huxley's 'highly important' book, *New Systematics*, as well as an address list for the Society's 374 members and a colossal 1,250-item bibliography listing relevant papers since 1938. The bibliography provides a fascinating cross section of research into speciation during the late 1930s, organised in ways Emerson thought appropriate for this fast developing topic.

Viewed from a distance, the Society was off to a strong start. But such optimism was mistaken. Emerson's first communication was the Society's last. By 1942, it was dead.

Regular contact with anyone interested reprints these little known documents Emerson circulated in 1941. These are the first and only materials distributed on behalf of the Society for the Study of Speciation. Few copies have survived the intervening years, and these have been used only rarely by historians interested in evolutionary theory. To these original documents, Cain has added a brief introduction as well as some clarifying notes and a summary of his detailed analysis of the Society's membership.

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